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OF THE

MEDICAL AND PHYSICAL SCIENCES.

SUPPORTED BY AN ASSOCIATION OF PHYSICIANS,

AND

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"In the four quarters of the globe, who reads an American book? or goes to an American play? or looks at an American picture or statue? *What does the world yet owe to American Physicians or Surgeons?*"

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M.L.A.

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CASES.

REVIEWS.

TO READERS AND CORRESPONDENTS.

1. We wish it to be distinctly understood, that we neither have, nor will receive, any pecuniary compensation as Editor of this Journal. The motives which led us to engage in the enterprize, are announced in our prospectus, and will be found liberal, and wholly disinterested. To this subject attention is now called, with a request, that communications for the work, and all matters of correspondence relative to it, may be addressed to the publishers, Messrs. H. C. Carey and I. Lea, Booksellers, Philadelphia.

2. For several very interesting communications for this Journal, we tender our acknowledgments, with a promise, that they shall appear in our next number.

3. We have received from Professor Caldwell, of Lexington, a copy of a Syllabus to his Course of Lectures on the Institutes of Medicine. The views which he takes of the subject are copious, original, and profound. Though it will prove somewhat difficult to convey, by any analysis, an adequate idea of the work, we purpose attempting it in our next number.

4. We are much obliged to Professor Beck, for the copy of his Treatise on Medical Jurisprudence, which he has kindly forwarded to us. The expectations excited by the annunciation of the work, we think, have been fully realised. It is, in our deliberate opinion, the most valuable production on the subject—honoorable to the medical literature of our country, and may be studied with advantage by the lawyer, physician, and general reader. As soon as we have leisure for the undertaking, we shall give to our readers some account of its contents, with the grounds of our approbation.

5. "Essays on Midwifery" have been just published by Dr. Dewees. We shall very soon review the work. But, in the meantime, we cannot forbear strongly recommending it to the medical public, as a most valuable production on the various and important subjects of which it treats.

6. We thank, very cordially, Professor Hosack, for his "Tribute to the late Dr. Rush," which is contained in our present number Though a mere sketch, it faithfully presents the broad lineaments of the character and services of that great man. We entirely concur with the eloquent biographer, that differing, as we may, as to the exact worth of some of his peculiar doctrines and modes of practice, we shall all unite on a candid estimate of his merits, in pronouncing him a distinguished benefactor to medicine, who has bequeathed to the School of Pennsylvania in particular, and to our country in general, the influence of an illustrious name.

"Clarum et venerabile nomen.
Gentibus, et multum nostræ quod proderat urbi."

7. The Medical Class in the University of Pennsylvania, at the very opening of the session, exceeds four hundred, and probably will be the most numerous the school has ever known.

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THE deeply interesting nature of the ensuing paper, more especially at the present period, when large sections of our country are pervaded by miasmatic diseases, induces us to deviate from a rule, which we have hitherto pretty strictly adhered to, of confining the contents of this Journal to original matter. The views which it presents of the subject, novel as they may appear to many, we have reason to suppose are substantially correct—and with some exceptions, are such as we have endeavoured to inculcate for several years, and when we thought them peculiar to ourselves. The work from which we extract the article has a very limited circulation in the United States, few copies being imported, and these deposited in libraries to which none of our country readers have access.—EDITOR.

[From the Transactions of the Royal Society of Edinburgh, vol. ix.]

ART. I. *On the Nature and History of the Marsh Poison.* By WILLIAM FERGUSON, M. D. F. R. S. Inspector of Army-Hospitals. Read January 3, and 17, 1820.

IN this paper I propose submitting to the Society some observations on the nature and history of the marsh poison, which, under the title of *marsh miasmata* or *malaria*, has ever been acknowledged as the undisputed source of inter-

mittent fevers, and is believed, with good reason, to be the exciting cause of the whole tribe of remittent fevers;—of endemic fever, in fact, in every form, and in every part of the world.

All authors who have treated of the nature of this poison, (and they are most numerous), coincide in attributing its deleterious influence to the agency of vegetable or aqueous putrefaction. So universal a coincidence has caused these opinions to be received with the authority of an established creed. It is my intention in this paper to show, from a narrative of facts, that they are *unfounded*, and that putrefaction, under any sensible or discoverable form, is *not* essential to the production of pestiferous miasmata.

The marsh poison, happily so little known in this country, and the colder regions of the earth, is notwithstanding by far the most frequent and destructive source of fever to the human race; as that form of fever to which it gives rise, rages throughout the world wherever a marshy surface has been exposed for a sufficient length of time to the action of a powerful sun. I have said for a sufficient length of time; because, as will presently be seen, the marsh must cease to be a marsh, in the common acceptation of the word, and the sensible putrefaction of water and vegetables must alike be impossible, before its surface can become deleterious. It will also be seen, that a healthy condition of soil, in these pestiferous regions, is infallibly regained by the restoration of the marshy surface in its utmost vigour of vegetable growth and decay. The previous marshy surface, or rather the previous abundance of water, is, however, an indispensable requisite preliminary, in all situations, to the production and evolvement of the marsh poison. A short review of the circumstances, which, under my own observation, attended our armies on service during the last war, will, I hope, render these seemingly paradoxical opinions intelligible to the Society.

The first time that I saw endemic fever, under the intermittent and r mittent forms, become epidemic in an army, was in the year 1794, when, after a very hot and dry summer, our troops, in the month of August, took up the

encampments of Rosendaal and Oosterhout, in South Holland. The soil in both places was a level plain of sand, with a perfectly dry surface, where no vegetation existed, or *could* exist, but stunted heath plants: on digging, it was universally found to be percolated with water to within a few inches of the surface, which, so far from being at all putrid, was perfectly potable in all the wells of the camp. I returned to Holland in the year 1799, with the army under the command of the Duke of York, which remained the whole autnmnal season in the most pestiferous portion of that unhealthy country, without its suffering in any remarkable degree from the endemic fever. Dysentery was almost the only serious disease they encountered. Remittent fever was nearly unknown, and intermittent occurred very rarely; but the preceding summer season had been wet and cold to an unexampled degree; during the whole of the service we had constant rains, and the whole country was one continuous swamp, being nearly flooded with water. In the year 1810, a British army at Walcheren, on a soil as similar as possible, and certainly not more pestiferous, but under the different circumstances of a hot and dry preceding summer, instead of a wet and cold one, suffered from the endemic fever of the country to a degree that was nearly unprecedented in the annals of warfare.

As I intend, in another part of this paper, to treat fully of the nature of the localities in the West Indies, I shall pass over at present my next experience of endemic fever during three years service in the Island of St. Domingo, and proceed to state what I observed on this subject in Portugal and Spain. In the course of the Peninsular war, during the autumnal campaign of 1808, our troops, after the battle of Vimeira, were comparatively healthy. The soil of the province around Lisbon, where they were quartered, is a very healthy one, (a slight covering of light sandy soil on a substratum of hard rock, which is almost always so bare, that water can seldom be absorbed into it to any depth, but is held up to speedy evaporation.) The season was fully as hot a one as is ordinarily seen in that country, but dysen-

tery was the prevailing disease. Early in 1809 the army advanced to Oporto, for the expulsion of the French under Marshal Soult from Portugal, which, during a very cold and wet month of May, (for that country,) they effected, without suffering any diseases but the ordinary ones of the bivouac; and in June advanced again towards Spain, in a healthy condition, during very hot weather. The army was still healthy, certainly without endemic fever, and marching through a singularly dry rocky country, of considerable elevation, on the confines of Portugal. The weather had been so hot for several weeks as to dry up the mountain-streams; and in some of the hilly ravines, that had lately been water-courses, several of the regiments took up their bivouac, for the sake of being near the stagnant pools of water that were still left amongst the rocks. The staff officers, who had served in the Mediterranean, pointed out the dangerous nature of such an encampment; but as its immediate site, amongst dry rocks, appeared to be quite unexceptionable, and the pools of water in the neighbourhood perfectly pure, it was not changed. Several of the men were seized with violent remittent fever before they could move from the bivouac the following morning; and that type of fever, the first that had been seen on the march, continued to affect that portion of the troops exclusively for a considerable time. Till then it had always been believed amongst us, that vegetable putrefaction (the humid decay of vegetables), was essential to the productions of pestiferous miasmata; but, in the instance of the half-dried ravine before us, from the stony bed of which, (as soil never could lie for the torrents,) the very existence even of vegetation was impossible; it proved as pestiferous as the bed of a fen. The army advanced to Talavera through a very dry country, and in the hottest weather fought that celebrated battle, which was followed by a retreat into the plains of Estremadura, along the course of the Guadiana river, at a time when the country was so arid and dry, for want of rain, that the Guadiana, itself, and all the smaller streams, had in fact *ceased to be streams*, and were no more than *lines* of

detached pools in the courses that had formerly been rivers; and there they suffered from remittent fevers of such destructive malignity, that the enemy, and all Europe believed, that the British host was extirpated; and the superstitious natives, though sickly themselves, unable to account for disease of such uncommon type amongst the strangers, declared they had all been poisoned by eating the mushrooms, (a species of food they hold in abhorrence,) which sprung up after the first autumnal rains, about the time the epidemic had attained its height. The aggravated cases of the disease differed little or nothing from the worst yellow fevers of the West Indies; and in all the subsequent campaigns of the Peninsula, the same results uniformly followed, whenever, during the hot seasons, any portion of the army was obliged to occupy the arid encampments of the level country, which at all other times were healthy, or at least unproductive of endemic fever.

To save further narrative, I shall finish this part of the subject, by adducing some topographical illustrations.

The bare hilly country near Lisbon, where the foundation of the soil, and of the beds of the streams is rock, with free open water-courses amongst the hills, as I have said before, is a very healthy one; but the Alentejo land, on the other side of the Tagus, though as dry superficially, being perfectly flat and sandy, is as much the reverse as it is possible to conceive. The breadth of the river, which at Lisbon does not exceed two miles, is all that separates the healthy from the unhealthy region; and the villages or hamlets that have been placed along the southern bank of the Tagus, for the sake of navigation, are most pestiferous abodes. The sickly tract, however, is not confined to the immediate shore of the river. Salvaterra, for example, about a mile inland, is a large village, and royal hunting residence in the Alentejo, which is always reputed to be very healthy till the beginning of the autumnal season, when every person, who has the means of making his escape, flies the place. In their superstitious fear, the inhabitants declare, that even the horses and other animals would be

seized with fever if left behind, and therefore they always remove the royal stud. The country around is perfectly open, though very low, and flooded with water during the whole of the rainy season; but at the time of the periodical sickness it is always most distressingly dry; and exactly in proportion to the previous drought, and consequent dryness of soil, is the *quantum* of sickness. I have visited it upon these occasions, and found it the most parched spot I ever saw. The houses of the miserable people that were left behind being literally buried in loose dry sand, that obstructed the doors and windows.

Cividad Rodrigo affords another illustration of the same. It is situated on a rocky bank of the river Agueda, a remarkably clear stream; but the approach to it on the side of Portugal is through a bare open hollow country, that has been likened to the dried-up bed of an extensive lake; and upon more than one occasion, when this low land, after having been flooded in the rainy season, had become as dry as a brick-ground, with the vegetation utterly burned up, there arose fevers to our troops, which for malignity of type could only be matched by those before mentioned on the Guadiana.

At the town of Corea, in Spanish Estremadura, not very dissimilarly situated, on the banks of the Alagon (also a very pure and limpid stream,) our troops experienced similar results; with this addition clearly demonstrated, that no spot of the pestiferous savannah below the town, was so much to be dreaded as the immediate shores of the river; so that even the running stream itself, which in all other countries has been esteemed a source of health, and delight and utility, in these malarious lands proved only an addition to the endemic pestilence. It is difficult to conceive any thing more deceptious than the appearance of these two towns, particularly the last, which might have been pitched upon by the best instructed medical officer, if unacquainted with the nature of malaria, as a place of refuge from disease; for the shores of the river, (it had no confining banks,) seemed perfectly dry, and there was not an aquatic weed,

nor a speck, nor line of marsh, to be seen within *miles* of the town, nor any thing but dry, bare, and clean savannah. It had, however, been so far the contrary in all past times, that the canons and ecclesiastics of its ancient cathedral had a dispensation from the Pope, of no less than five months leave of absence, to avoid the Calentura, (their name for the endemic fever.) In the other ecclesiastical residences of Estremadura, the same dispensation rarely extended beyond *three* months, but almost all had some indulgence of the kind. During the autumnal season, the epidemic prevailed so generally amongst all classes of inhabitants, that even infants at the breast were affected with it, and few of the residents attained to any thing like old age. The oldest person I ever saw in Corea, who was a priest, that had often taken advantage of the dispensation for leave of absence, was only in his 57th year, and he appeared like a man past 70. The inhabitants, nevertheless, seemed always surprised and offended when we condoled with them on the unhealthiness of their country, which they would not admit in any degree; for with them, as every where else, where immemorial experience has shown that it is impossible to avoid a calamity, it goes for nothing. They contemplated its approach with the same indifference that a Turk does the plague, and patiently awaited its extinction by the periodical rains of the winter season, not, however, without some exultation and self-congratulation, on the greater comparative mortality that occured amongst the stranger soldiers than amongst themselves.

From all the foregoing, it will be seen, than in the most unhealthy parts of Spain, we may in vain, towards the close of the summer, look for lakes, marshes, ditches, pools, or even vegetation. Spain, generally speaking, is then, though as prolific of endemic fever as Walcheren, beyond all doubt one of the driest countries in Europe, and it is not till it has again been made one of the wettest, by the periodical rains, with its vegetation and aquatic weeds restored, that it can be called healthy, or even habitable, with any degree of safety.

During the years 1815, 1816, and 1817, I was employed in making a topographical health survey of all the West India colonies, which afforded me opportunities, in that diversified, dangerous, and active climate, of improving the observations I had elsewhere made upon pestiferous miasmata, of a kind that I could scarcely have anticipated.

It might *there* be seen, that the same rains which made a deep marshy country perfectly healthy, by deluging a dry well cleared one, where there was any considerable depth of soil, speedily converted it, under the drying process of a vertical sun, into a hot-bed of pestiferous miasmata. Thus, in the Island of St. Lucia, the most unwholesome town of Castries, at the bottom of the Carenage, which is altogether embosomed in a deep mangrove fen, became perfectly healthy under the periodical rains; while the garrison, on the Hill of Morne Fortuné, immediately above it, within half cannon shot, began to be affected with remittent fevers. The two localities within this short distance evidently changed places in respect to health. The top and shoulders of the hill had been cleared of wood, and during a continuance of dry weather, the garrison had no source of disease within itself, but this was amply, though but temporarily supplied, as soon as the rains had saturated the soil on which it stood. Thus an uncommonly rainy season at Barbadoes, seldom failed in that perfectly dry and well-cleared country, to induce for a time general sickness; while at Trinidad,—which is almost all swampy, and the centre of the island may be called a sea of swamp, where it always rains at least nine months in the year,—if it only rained eight, or if at any time there was a cessation of the preserving rains, the worst kind of remittent fevers were sure to make their appearance. General dryness of soil, however, is far from being the ordinary characteristic of our West India colonies. The swamp is too often exposed to the continued operation of a tropical sun, and its approach to dryness is the harbinger of disease and death to the inhabitants in its vicinity. On the whole, it may truly be said, that although excessive rains will evidently cause the acknowledged wholesome and un-

wholesome soils to change places for a time, in respect to health, a year of stunted vegetation, through dry seasons, and uncommon drought, is infallibly a year of pestilence to the greater part of the West India colonies.

In some other respects, the history of miasmata in these countries was curious and interesting. Thus at the town of Point au Pitre, Guadaloupe, which is situated amidst some of the most putrid marshes in the world, the stench of which is almost never absent from the streets, the place was far from being *uniformly* unhealthy. Strangers, however much they might be annoyed with the smell, often resorted to it with impunity. No more was its first out-post fort Louis, where the waters are so stagnant and putrid, that it is even more offensive than Point au Pitre; but at Fòrt Fleur d'Epeé, the farthest out-post, at the extremity of the marshes, where they approach to the state of Terra Firma, where little or no water is to be seen on the surface, and no smell exists, there cannot be supposed a more deadly quarter, and all white troops considered their being sent there, as an equivalent to a sentence of death. It ought to be noted, that the marshes of all these three posts are overgrown with the thickest underwoods, and rankest aquatic vegetation of every kind. A fact of the same kind has been observed in the Island of Tobago. The principal fort and barrack of the colony, has been placed immediately to leeward of the Bacolette swamp, within the distance of less than half a mile, and the strong ammoniacal stench of its exhalations, even at that distance, often pollutes the barracks; but these are so far from producing fever at all times, that when I visited the white garrison there, they had been more remarkably exempt from that form of disease, for several years, than any other troops in the West Indies. I shall not multiply facts and illustrations of the same kind, to prove that putrefaction, and the matter of disease, are altogether distinct and independent elements; that the one travels beyond the other, without producing the smallest bad effect; and that, however frequently they may be found in company, they have no *necessary* connection; but proceed to notice other

qualities of the marsh poison, which, until understood, prove extremely puzzling to the observer.

In selecting situations for posts and barracks, it had been observed with surprise, that the border, and even the centre of the marsh, proved a less dangerous quarter than the neighbouring heights of the purest soil, and healthiest temperature; and this has never been more strongly exemplified than in the instances I am going to relate.

Port of Spain, Trinidad, the capital of the island, is situated very near the great eastern marsh, with which it is in direct communication, by a marginal line of swamp along the sea-shore. It cannot be called a healthy town, but it is very far from being uninhabitable. On the right are some covering heights, which rise out of the marsh at one extremity. These, unlike the site of the town, which has been built on marshy or alluvial ground, are composed of the purest and most healthy materials,—(pure limestone, the purest and the best in all the West Indies), yet have they proved a residence deadly and destructive in the greatest degree to all who venture to inhabit any part of their diversified surface. No place, however elevated, or sunk, or sheltered, or walled in, gives security against the exhalations from below, only it has been distinctly ascertained, that these prevail with more or less malignity, exactly in proportion to the elevation of the dwelling. The lower, consequently the *nearer* the marsh, the better. The tops of the ridges are uninhabitable. On the highest top, at an elevation of 400 feet, and farther removed from the marsh than the town itself, a large martello tower was built to defend the place. It possessed a fine temperature, but proved so dangerous a quarter, that it was obliged to be abandoned. Not even a creole mulatto Spaniard could sleep in it with impunity for a single night, after a course of dry weather.

The beautiful post of Prince Rupert's, in the Island of Dominica, is a peninsula which comprehends two hills of a remarkable form, joined to the main land by a flat, and very marshy square isthmus to windward, of about three quar-

ters of a mile in extent.* The two hills jut right out on the same line into the sea, by which they are on three sides encompassed. The inner hill, of a slender pyramidal form, rises from a narrow base nearly perpendicular, above and across the marsh, from sea to sea, to the height of 400 feet, so as completely to shut it out from the post. The outer hill is a round-backed bluff promontory, which breaks off abruptly, in the manner of a precipice, above the sea. Between the two hills runs a very narrow clean valley, where all the establishments of the garrison were originally placed; the whole space within the peninsula being the driest, the cleanest, and the healthiest surface conceivable. It was speedily found that the barracks in the valley were very unhealthy, and to remedy this fault, advantage was taken of a recess or platform near the top of the inner hill, to construct a barrack, which was completely concealed by the crest of the hill from the view of the marsh on the outside, and at least 300 feet above it; but it proved to be pestiferous beyond belief, and infinitely more dangerous than the quarters in the valley, within half musket-shot below. In fact no white man could possibly live there, and it was obliged to be abandoned. At the time this was going on, it was discovered, that a quarter which had been built on the outer hill, on nearly the same line of elevation, and exactly 500 yards further removed from the swamp, was perfectly healthy, not a single case of fever having occurred on it from the time it was built. These facts were so curious, that I procured the surveyor-general of the island to measure the elevations and distances, and I have given them here from his report.

In the Island of Antigua, the same results were confirmed in a very striking manner. The autumn of 1816 became very sickly, and yellow fever broke out in all its low marshy quarters, while the milder remittent pervaded the island generally. The British garrison of English Harbour soon

* The superficies of the base of the peninsula is exactly 1210 yards in length, by 850 in breadth, exclusive of the isthmus.

felt the influence of that most unwholesome place. They were distributed on a range of fortified hills that surround the dock-yard. The principal of these, Monk's Hill, at the bottom of the bay, rises perpendicular above the marshes to the height of 600 feet. The other garrisoned hill, which goes by the name of the Ridge, is about 100 feet lower, but instead of rising perpendicularly, it slopes backwards from the swamps of English Harbour. It was the duty of the white troops, in both these forts, to take the guards and duties of the dock-yard amongst the marshes below, and so pestiferous was their atmosphere, that it often occurred to a well-seasoned soldier mounting the night-guard in perfect health, to be seized with furious delirium while standing sentry, and when carried back to his barracks, on Monk's Hill, to expire in all the horrors of the black vomit, within less than 30 hours from the first attack; but during all this, not a single case of yellow fever, nor fever of any kind, occurred to the inhabitants of Monk's Hill; that is to say, the garrison staff, the superior officers, the women, the drummers, &c., all in fact that were not obliged to *sleep* out of the garrison, or take the duties below, remained in perfect health. The result on the Ridge was not quite the same, but it was equally curious and instructive. The artillery soldiers, (17 in number) never took any of the night guards, but they occupied a barrack about 300 feet above the marshes, not perpendicular above them, like Monk's Hill, but a little retired. Not a case of yellow fever or black vomit occurred amongst them, but every man, without a single exception, suffered an attack of the ordinary remittent, of which one of them died; and at the barrack on the top of the Ridge, at the height of 500 feet, and still further retired from the marshes, there scarcely occurred any fever worthy of notice.

Another property of the marsh poison is, its attraction for, or rather its adherence to, lofty umbrageous trees. This is so much the case, that it can with difficulty be separated from them; and in the territory of Guiana particularly, where these trees abound, it is wonderful to see how near *to lee-*

ward of the most pestiferous marshes the settlers, provided they have this security, will venture, and that with impunity, to place their habitations.

The localities of the plantations situated on the windward banks of the rivers that intersect Guiana, and are generally covered by swampy woods in close vicinity, exemplify this fact in a remarkable manner; and at Paramaribo, the capital of Surinam, the trade-wind that regularly ventilates the town, and renders it habitable, blows over a considerable tract of swamp at a short distance, but which, fortunately for the inhabitants, is thickly covered with umbrageous forests. Experience, besides, has shown, that there, as in all other new lands, the cutting down of those trees in the swamps has ever been a fatal operation in itself, and in all probability would be productive of pestilence to the town.*

It would be trespassing wantonly on the time and patience of the society, to multiply further observations of the same kind, and I shall therefore proceed to draw some conclusions, which I think are fairly warranted from the facts and narrative I have submitted.

That the marsh poison cannot emanate from vegetable putrefaction, I think must be evident from the fact, that it is found most virulent and abundant on the driest surfaces; often where vegetation never existed, nor *could* exist for the torrents, such as the deep and steep ravine of a dried water-course, and that it is never found in savannahs or plains, that have been flooded in the rainy season, till their surface has been thoroughly exsiccated; vegetation burnt up; and its putrefaction rendered as impossible as the putrefaction of an Egyptian mummy. If this be doubted or denied, let us take examples where vegetable putrefaction is self-evi-

* The town of New Amsterdam, Berbice, is situated within short musket-shot to leeward of a most offensive swamp, in the direct tract of a strong trade-wind, that blows night and day, and frequently pollutes even the sleeping apartments of the inhabitants, with the stench of the marshes, yet it had produced no endemic fever worthy of notice, even amongst the newly arrived, for a period of months and years previously to my visiting that colony.

dent, and examine whether it be productive of disease and death, similar to what emanates from the marsh poison. Surely the evidence of every dung heap, in every part of the world, will answer the question in the negative; or if it be insisted that the poison is generated from a combination of aqueous and vegetable putrefaction, let us resort to the easy familiar illustration of a West India sugar-ship, where the drainings of the sugar, mixing with the bilge-water of the hold, creates a stench that is absolutely suffocating to those unaccustomed to it; yet fevers are never known to be generated from such a combination. These are familiar examples; but I cannot think they should be of less intrinsic value on that account, or be deemed less conclusive. The Italians, to be sure, have published ordinances against the steeping of hemp in stagnant pools, but these resemble many other ordinances relative to health every where; in overlooking the leading primary causes of the stagnant pool, the autumnal season, and the malarious lands around, and having their point directed to a trifling concomitant circumstance of no importance.

Should it be said, that the poison must then emanate from aqueous putrefaction alone, I think this may be disproven by equally familiar examples. The bilge-water in the holds of ships, which at all times smells more offensively than the most acknowledged pestiferous marshes, would in that case infallibly, and at all times, be generating fevers amongst the crew, more particularly in tropical climates. I need scarcely say, that this does not consist with the fact, unless it be in some rare instances, where the bilge-water has become, like that of the marsh, actually dried up, or absorbed into the collected rubbish and foulness of the ship's well, thereby verifying the common saying of the sailors, that a leaky ship is ever a healthy ship, and *vice versa.* Or if it be objected, that the salt may have a preserving power, let us look at the quantity of fresh-water, (not unfrequently the impure water of an alluvial river), laid in for a first-rate man of war proceeding on a long voyage. This is so great as to constitute many floorings, or tiers of barrels, close to which the

people sleep with impunity, though it is always disgustingly putrid, and could not fail to affect them, if it contained any seeds of disease.* Examples of the same on land may be found with equal facility. At Lisbon, and throughout Portugal, there can be no gardens without water; but the garden is almost every thing to a Portuguese family. All classes of the inhabitants endeavour to establish and preserve them, particularly in Lisbon, for which purpose they have very large stone reservoirs of water, that are filled by pipes from the public aqueducts, when water is abundant;—but these supplies are always cut off in the summer. The water, consequently, being most precious, is husbanded with the utmost care for the three months absolute drought of the summer-season. It falls of course into the most concentrated state of foulness and putridity, diminishing and evaporating day after day, but never absorbed, till it subsides either into a thick green vegetable scum, or a dried crust. In the confined gardens of Lisbon particularly, these reservoirs may be seen in this state close to the houses, even to the sleeping places of the household, in the atmosphere of which they literally live and breathe; yet no one ever heard or dreamt of fever being generated amongst them from such a source; though the most ignorant native is well aware, that were he only to cross the river, and sleep on the sandy shores of the Alentejo, where a *particle* of water at that season had not been seen for *months*, and where water being absorbed into the sand as soon as it fell, was *never* known to be *putrid*, he would run the greatest risk of being seized with the remittent fever.

* In some ships of our navy, the fresh water, instead of being put in casks, has been preserved in bulk, by constructing a large open tank, of tin or lead, at the bottom of the hold, without in the least affecting the health of the crew, though they slept immediately above it. On land, the very same results have been verified under the same circumstances. One of the healthiest officers'-quarters in the West Indies, is the field-officers at Berkshire Hill, St. Vincent's, which is built immediately over the garrison water-tank; and a block house at Demerara, similarly situated, was healthier than the other posts on *terra firma.*

From all the foregoing, the deduction appears to be unquestionable, that endemic fevers cannot be generated either from aqueous or vegetable *putrefaction*, singly or combined. It emanates, as we have seen, from the shores of the purest streams, wherever they have been flooded during the rains, through want of confining banks, and it is absent from the most putrid waters. It must be impossible that healthy living water, which from its current is in a perpetual course of being refreshed and renewed, can ever, by any degree of solar heat, be brought into the state of morbific miasmata; and the evil must therefore reside in the half-dried and drying margin; for the swamp is no more than this margin rolled up under another shape, and it must be brought into the same degree of *dryness*, before it can produce any morbific effects.

One only condition then seems to be indispensable to the production of the marsh poison, on all surfaces capable of absorption; and that is, the *paucity* of water, where it has previously and recently *abounded*. To this there is no exception in climates of high temperature; and from thence we may justly infer, that the poison is produced at a highly advanced stage of the *drying* process;—but, in the present state of our knowledge, we can no more tell what that precise stage may be, or what that poison actually is, the development of which must necessarily be ever varying, according to circumstances of temperature, moisture, elevation, perflation, aspect, texture, and depth of soil, than we can define and describe those vapours that generate typhus fevers, small-pox, and other diseases. The marsh and the stagnant pool will no doubt be pointed out as the ostensible sources from which this poison has ever sprung; but the marsh, it has been seen, is never pestiferous when fully covered with water. At all other times it must present a great variety of drying surface, and both the lake and the marsh must ever possess their saturated, half-dried, and drying margins. It is from these that the poison uniformly emanates, and never from the body of the lake or pool, and I think it may even be fairly presumed that water, for as

long as it can preserve the figure of its particles above the surface, is innoxious, and that it must first be absorbed into the soil, and disappear to the eye, before it can produce any mischievous effects. The most ignorant peasant of Lincolnshire knows, that there is nothing to be apprehended from the ditches of his farm till they have been dried up by the summer heat; and though the inhabitant of Holland may point to the unexhausted foul canal as the source of his autumnal fever, there can be little doubt that he might live upon a sea of the same with impunity, and that it is to the absorbed waters under his feet, which, without the canal, would in all probability be much more pestilential, he ought to attribute his disease. To assert, after all this, that the putrid marsh, which must necessarily, to a certain degree, be a *wet* one, is positively less dangerous than another where no smell exists, will not, I am sure, appear paradoxical to the society; for it is only saying, that the first has not yet arrived at the degree of exsiccation that has been found most productive of the marsh poison, and that putrefaction, though it may, and must often precede and accompany pestilence, is no part of pestilence itself.

The symbol of vegetable putrefaction, in the decay of the aquatic weeds that cover the pool, constantly meets the eye, and deceives the judgment; and the smell of the putrefying waters combined in it, confirms a delusion which has ever prevented us from discovering, that the action of a powerful sun on its half dried margin, is adequate to the production of all that could be attributed to the humid decay of vegetables. The greatest danger then may, and does often exist, where no warning whatever is perceptible to the senses, and whoever, in malarious countries, waits for the evidence of putrefaction, will, in all the most dangerous places, *wait too long*, as every one can testify who has seen pestilence steam forth, to the paralyzation of armies, from the bare barren sands of the Alentejo in Portugal,—the arid burnt plains of Estremadura in Spain,—and the recently flooded table-lands of Barbadoes, which have seldom more than a foot of soil to cover the coral rock, and are therefore, under

the drying process of a tropical sun, brought almost immediately after the rains into a state to give out pestilential miasmata.

I shall conclude this paper with a few more observations on some of the qualities not yet noticed of the marsh poison. No experiments hitherto made have enabled us to pronounce whether it be specifically heavier or lighter that common air; but it evidently possesses an uncommon and singular attraction for the earth's surface; for in all malarious seasons and countries, the inhabitants of *ground floors* are uniformly affected in a greater proportion than those of the *upper stories*. According to official returns during the last sickly season at Barbadoes, the proportion of those taken ill with fever, in the lower apartments of the barracks, exceeded that of the upper by one-third, throughout the whole course of the epidemic. At the same time it was observed, that the deep ditches of the forts, even though they contained no water, and still more the deep ravines of rivers and water-courses, abounded with the malarious poison. At Basseterre, Guadaloupe, a guard-house placed at the conflux of the inner and outer ditch of the fort, infallibly affected every white man with fever that took a single night-guard in it; and the houses that were built in the ravine of the river *aux herbes* (a clear rapid mountain-stream that runs through the town) or opposite to its "bouchure," proved nearly as unhealthy as the guard-house above mentioned.

Another proof, that from the attraction above mentioned it creeps along the ground, so as to concentrate and collect on the sides of the adjacent hills, instead of floating directly upwards in the atmosphere, is the remarkable fact, *that it is certainly lost and absorbed by passing over a small surface of water;* which could scarcely happen, unless it came into direct contact with the absorbing fluid. The rarefying heat of the sun, too, certainly dispels it, and it is only during the cooler temperature of the night that it acquires body, concentration, and power. All regular currents of wind have also the same effect, and I conceive it to be through

the agency of the trade wind alone, which blows almost constantly from east to west, that the greater part of the West Indies is rendered habitable. When this purifying influence is withheld, either through the circumstances of season, or when it cannot be made to sweep the land on account of the intervention of high hills, the consequences are most fatal. The leeward shore of Guadaloupe, for a course of nearly thirty miles, under the shelter of a very high steep ridge of volcanic mountains, never felt the *sea*-breeze, nor any *breeze* but the night land-wind from the mountains; and though the soil, which I have often examined, is a remarkably open, dry, and pure one, being mostly sand and gravel, altogether and positively without marsh in the most dangerous places, it is inconceivably pestiferous throughout the whole tract, and in no spot more so than the bare sandy beach near the high water-mark.* The coloured people alone ever venture to inhabit it, and when they see strangers tarrying on the shore after night-fall, they never fail to warn them of their danger. The same remark holds good in regard to the greater part of the leeward coast of Martinique, or the leeward alluvial basis of hills, in whatever part of the torrid zone they may be placed, with the exception probably of the immediate sites of towns, where the pavements prevent the rain-water being absorbed into the soil, and hold it up to speedy evaporation.

For this, if there *be* a remedy, it must be found in the powers of cultivation, ever opening the surface for the es-

* In our own country, an instance of pure surface, absolutely destitute of vegetation, proving as malarious as any other spot that I know of in England, may be seen at Dungeness, on the coast of Kent. The point of Dungeness, is a tongue of land appended to the great Romney Marsh, and consists of an extensive bank of shingle or gravel, so dry, loose, and open, that even during wet weather horses sink in it nearly up to the knees. The forts and barracks are at least four miles from what may be called the Mainland, where the grass begins to grow, yet was there no spot of that unwholesome tract of country more prolific of endemic fever during the hot summer and autumn of 1807 than these barracks. In one part of the gravel, but not near the barracks, were some very deep pools, of no great extent, containing a singularly pure pellucid fresh-water.

cape of pestilential gases, and exhausting the morbific principle by a constant succession of crops; for wherever malaria prevails, the uncultivated savannah, even though used for pasture, becomes infinitely more pestiferous than the plantation, and the depopulated country falls completely under its dominion. With the aid of the purefying sea-breeze, this course, at the British colony of Demerara, within six degrees of the equator, has succeeded in rendering the cultivated portion of the deepest and most extensive morass probably in the world, a healthy, fertile, and beautiful settlement. I shall not here enter into a detailed account of the astonishing system of tide and flood-gate drainage by which this delightful result has been established and kept up, but hasten to a conclusion.

It would be unphilosophical to suppose, that the marsh poison, because other distempers, such as dysentery, coexist with it, ever produces any disease but the specific one of which it is the acknowledged parent, varying, however, in form, and as a modification of effect from the same cause, from the common ague of the fens of Lincolnshire, through all the milder remittent types, up to the aggravated yellow fever, or malignant remittent of the West Indies; and *that* variation, so certain and uniform, in proportion to the power of the remote exciting cause, that the varying types of fever might be measured almost to a certainty by the degrees of solar heat, as marked on the thermometer. Thus it is most rare and uncommon to meet with an ague in the West Indies in the swampy alluvial plains at the level of the sea, where the generality of the towns and settlements are placed;—as rare, or rather as impossible, as it would be to meet with any thing else but a common remittent or an intermittent fever, on the cooler mountain marshy levels of the same country. The highest degree of susceptibility and excitement from the solar heat on the part of the *subject*, combined with the highest state of preparation from the same, on the part of the *agent*, appear to be essential in all situations to the production of the dreadful yellow fever, which, luckily for mankind, is incapable of being transported

to any locality of lower temperature, or texture of soil different from that which gave it birth. Need I say, that such a disease, however rapid and appalling may be its epidemic current, is not, and cannot, no more than the common ague or remittent fever, be in the smallest degree contagious.

There are very few, indeed, of those that have been compelled to live under its scourge, who have not overcome the prejudices of their education on this head; but, unluckily, a different impression obtains very generally amongst those who have never seen the disease, which deeply affects the peace of society every where, and in some countries has proved subversive of the best interests of humanity. Such opinions, it is the duty of every man, who has had sufficient experience, to combat, by stating the results of personal exposure and investigation, and thereby do his utmost to rescue medical science from the dominion of a prejudice which disgraces it.

I shall now close this tedious paper, wherein I have endeavoured, as much as possible, to avoid all professional disquisitions, or references to authors, or even allusion to any ground on which I have not personally trod. It was my earnest wish to have made it shorter; but amidst the multiplicity of matter and illustration, with which I have been literally oppressed, I found that I could not abridge it farther, and at the same time do justice to the subject.

NOTE.

On the Negro Skin.—The adaptation of the negro to live in the unwholesome localities of the torrid zone, that prove so fatal to Europeans, is most happy and singular. From peculiarity of idiosyncrasy, he appears to be proof against endemic fevers; for to him marsh miasmata are in fact no poison, and hence his incalculable value as a soldier, for field service, in the West Indies. The warm, moist, low and leeward situations, where these pernicious exhalations are generated and concentrated, prove to *him* congenial in every respect. He delights in them, for he there enjoys life and health, as much as his feelings are abhorrent to the currents

of wind that sweep the mountain tops, where alone the whites find security against endemic fevers.

One of the most obvious peculiarities of the negro, compared with the European, is the texture of his skin, which is thick, oily, and rank to a great degree: and from this circumstance the theorist, when he speculates on the mode of reception of the marsh poison into the constitution, whether by the lungs, the stomach, or the skin, may draw a plausible conjectural inference (for it can be no more) in favour of the last. It is certain, that amongst Europeans, the thick-skinned and dark-haired withstand the influence of the marsh poison much better than those of the opposite temperament; and it is equally certain, from the never-failing primary head-ache, that its first impression is invariably upon the brain, as if it had been taken up by the sentient extremities of nerves, of which the skin is so truly an expansion, and conveyed to the sensorium.

Another argument of analogy, in favour of the same opinion, may be derived from a reference to the plague, which is the pestilential endemic fever of the *Levant*, or rather of the arid sandy regions of the southern coasts of the Mediterranean. In that disease, we see reason to believe, that the poison enters by the skin, because swellings of the lymphatic glands are amongst its most prominent symptoms; because oily frictions on the skin are said to be preservative against it, and that carriers or workers in oil do not take the disease.

When the poison is received into the constitution, through whatever channel it may enter, its effects are actually not very dissimilar in some cases to those from the *bite of a serpent*. The aggravated cases of yellow fever at Antigua, mentioned in this paper, and those that have frequently occurred at St. Lucia and Martinique, from the bite of the large brown viper of these islands, ran a course not without some resemblance, in the impaired nervous energy, the vomitings, and dissolution of the blood, as marked by the livid discolorations under the skin, (hence the very improper name of yellow fever), and its discharge from the internal surfaces previous to the fatal termination.

Art. II. *Thoughts on Animal Heat*. By N. Chapman, M. D.

By most writers, respiration has hitherto been considered as consisting in a series of chemical actions—which, in my opinion, is not altogether a correct view of the subject. Whatever may be the degree of chemical agency in this process, it is subordinate, and as happens in every other function of the animal economy, controlled by the vital power. The lungs, instead of being mere passive receivers of the air inspired, are vital organs, and act upon it in a peculiar way. As was held by the ancients, respiration really seems to be a species of digestion, in which, that portion of air, that is appropriate to the exigencies of the animal economy, is employed, while the rest is rejected as excrementitious. The oxygen of our atmosphere is, therefore, emphatically the "pabulam vitæ."*

That the lungs act as vital organs, is shown by the fact, that if they be inflated with pure oxygen, no more of it is consumed than is contained in the atmosphere, or in other words, only twenty-seven parts in the hundred. To this purpose we have a variety of evidence, though, perhaps, none more conclusive than that afforded by the experiment so often tried, of animals subsisting nearly four times the period, when inclosed in a vessel of oxygen, that they do in one holding atmospherical air. It is also worthy of remark, that the consumption of oxygen, and the formation of carbonic acid, are uniformly proportioned to the vigour of the vital energies. Experiments prove, that the operation proceeds most actively in the morning, and that whatever exhausts the system, as inordinate exercise, the depressing passions, a mercurial course, or the cold stage of an intermittent, lessens the effect, exactly as happens with respect to digestion, the circulation, and other vital functions.† Nor does the analogy stop here. As in the preceding cases, it appears, that respiration is materially influenced by the nerves.

* Richerand's Physiology. † Murray's Chemistry, Vol. II. p. 488.

It has long been known, that the process of digestion might be suspended, or prevented entirely, by cutting or tying the eighth pair of nerves. By the recent experiments of Provencal, a distinguished French physiologist, we are taught, that this operation has precisely the same effect on respiration. The lungs cease at once to perform their office, and the blood is returned to the heart unchanged, in consequence of which, the animal speedily perishes, with all the symptoms of asphyxia. It is true, that the experiments of Brodie and Dupreten, as well as those of Le Galois, are calculated to show, that this is owing more to an interruption in the mechanism of respiration, than to any change in the action of the lungs on the blood. They allege, that by artificially inducing respiration, the florid colour is restored to the blood, and the action of the heart may be continued for some time. Even admitting the accuracy of these reports, they only prove, that life still lingers in the lungs, where such effects can be produced. Did not these organs, by virtue of their vitality, exert an influence on the air inhaled, totally different from chemical action, what is there to prevent resuscitation, under any circumstances, where inflation can be accomplished? Conceding, that the lungs are passive receivers, and respiration only a play of chemical affinities, the operation ought surely to proceed just as well in a dead as a living animal, while organization remains perfect.

Connected with respiration, and the changes which the blood suffers in that process, is the production of animal heat according to the views at present, very generally entertained. Nothing in physiology seems earlier to have attracted the attention of mankind, than the power possessed by the more perfect animals, of preserving an uniform temperature amidst the vicissitudes of heat and cold. By the ancients, the subject was treated with the greatest reverence, they viewing animal heat as an emanation from the Deity, and the peculiar property of vitality. As knowledge advanced, we are not therefore to be surprised if the attempts were many, to get at an explanation of this very interesting

function. What, however, was done by antiquity in this way, will be found to be mere conjectural absurdity, or weak deductions from remote analogies, having very slight pretensions to our notice. It was not till the modern improvements in chemistry, that any of the lights of science were thrown upon the subject. The production of animal temperature was, indeed, ascribed by Mayow to the action of the air in the lungs, without, however, the support of any decisive evidence. The discovery of Black of latent heat, has been commonly considered as the primary step in the progress of our real knowledge of the subject. But to Crawford we are indebted for the fullest elucidation of it, and for the promulgation of a theory that has attracted the largest share of attention, and commanded the most general confidence.

As is now sufficiently known, he maintains, that during respiration, oxygen combines with carbon, producing a species of slow combustion, in which a quantity of caloric is extricated. The blood is at the same time changed from venous to arterial, and by this change, acquires an increased capacity for caloric.* On this account it is enabled to take up the caloric set loose, so that any excess of temperature in the lungs which might be detrimental is prevented. The arterial blood is carried in the course of the circulation to the extreme vessels, where it passes into the venous state. By this conversion, its capacity for caloric is diminished as much as it had been previously increased in the lungs. The caloric, therefore, which it had imbibed is again released, and this constant evolution of it, in the extreme vessels, throughout the whole body, is the cause of its uniform temperature.

The superiority of this, over the rival theories on the same subject, consists altogether, in the alleged discovery

* The difference between the two kinds of blood he alleges to be as 1.030. to 0.892. That is to say, supposing the capacity of water for caloric to be 1000, the capacity of arterial blood is rather larger, or 1.030, while that of venous blood is considerably less, or only 892. Murray's Chemistry, p. 488.

of the different capacities for caloric in the two kinds of blood. None of the chemical speculations relative to animal heat, which assign its production to the disengagement of caloric in the lungs, explained, why it is not excessive in these organs, or how it comes to be every where equally diffused.

Most unhappily for this plausible hypothesis, it has been found, that there is no material difference in the capacities of arterial and venous blood, or of arterial blood and carbonic acid, and hence the very foundation on which it rests is subverted.* What, it may be demanded, becomes of the excess of caloric disengaged in the lungs, since neither arterial blood, nor carbonic acid, has a superior capacity for it? To this question no one has hitherto ventured an answer, and, till it is done, the theory remains wholly invalidated. But, independently of the preceding objections, the hypothesis is held to be defective, even by its admirers, inasmuch as it limits the generation of caloric to a single source. Besides the lungs, it is affirmed, that there are several avenues by which it is introduced into the body, that ought not to be overlooked in an exposition of this process. Of these, the skin is considered as one. Experiments made by Milley, Cruikshank, Abernethy, and Jurin, are said to show, that the air in contact with the surface, suffers changes so far, at least, like those it undergoes in respiration, as that a portion of oxygen disappears, and carbonic acid is formed. It is stated, that the consumption of oxygen in this case, is much regulated by the quantity of blood determined to the cutaneous vessels, and is increased by exercise, a high temperature, &c. Notwithstanding, however, the weight of authority I have cited on this point, it is very doubtful whether the skin performs the office alleged. Of this, Priestley was unable to satisfy himself, and we have the

* It is shown by Dr. John Davy, that the specific caloric of arterial blood is 0.913, and of venous blood 0.903. Delaroche and Berand have shown, that the specific heat of oxygen gas is 0.8848, and of carbonic acid gas 0.8280. Thomson's Chemistry, vol. iv. p. 477. Vid. Eclec. Repertory, p 188, vol. 6.

counter-experiments of Klapp and Gordon, which were apparently made with so much precision, as to claim no inconsiderable degree of confidence.*

As some oxygen is taken into the stomach with the food, it is presumed that a portion of caloric is evolved, as well as that there may be a slight addition derived from the air, which combines with the mucous secretions. Be this as it may, these three sources, which are all that have been pointed out, can afford very little sensible caloric, since even what is produced is, perhaps, nearly, or entirely expended in the respective operations going on, in the parts where the air is received. Thus, the caloric of the surface is carried off by perspiration, that of the stomach absorbed by the food, while that of the mucous secretions is employed in rendering these fluids thinner and more vapoury.

An infinitely greater laboratory of heat has been imagined to exist, in those chemical operations of the system, by which caloric is converted from a latent to a sensible state. We have already seen how much was ascribed, though without foundation, to the change of arterial into venous blood—and it is also maintained, that caloric is freely evolved in the conversion of the elements of the blood into the solids, since the latter have a less capacity for it than liquids, of the same chemical composition. But though a general, this is not an universal law. Caloric, on the contrary, is sometimes disengaged in the passage of solids into the fluid or vapoury state—one of the most striking examples of which, we have in the explosion of gunpowder. Granting, however, the principle, it really seems that, in its present application, it leads to a directly opposite conclusion from the one intended. Confessedly, the fluids of the body exceed the solids—and hence, there is a preponderance of those operations by which they are formed, and of necessity, a retention, instead of an evolution of temperature. As respects the process of secretion, so much insisted upon, as a source of heat caused by the more intimate union which is

* Murray's Chemistry, p. 493.

supposed to take place between the oxygen and carbon, we have in two instances, at least, that of the bile and fat, more copious than any other of the secretions, an actual separation of combustible matter from the blood.

As enumerated, such are the chief sources from which, according to chemical views, the living system is supposed to derive its heat. But as under all circumstances, whether placed among polar snows, or beneath a tropical sun, it preserves, when in a healthy state, the same degree of temperature, it becomes us next to trace the means by which it is accomplished. The temperature of the body being higher than that of the surrounding medium, the explanation is obvious. It is a well known law of caloric, that it will pass from a hot to a cold body, till an equilibrium of temperature is established. There is also a steady evaporation going on, as well from the whole cutaneous surface, as the lungs, by which it is copiously absorbed.

Not a little is, moreover, removed in the process of expiration. The air, in this state, is always of the temperature of the body, and therefore, if taken in colder, such an effect necessarily follows. Computations, apparently accurate, make the heat removed in this way alone, in twenty-four hours, at a temperature of 59°, sufficient to melt thirty pounds of ice.*

As yet, the precise quantity of caloric is not determined, which the body imparts to the surrounding medium, under given circumstances. But we are well informed that it is regulated:

1. By the greater or less difference between the temperature of the body and that of the medium. Thus a medium of 30° will carry off much more caloric than one of 60°.

2. By the conducting power of the medium. There is, in this respect, a very marked difference. Metals abstract caloric more speedily than stone—stone than glass—and glass than charcoal. Wool is also a bad conductor.

3. By the spontaneous motion of the medium. It is found

* Dr. Menzies.

that a stream of air, or water, draws off caloric with greater rapidity than when these fluids are at rest. Either a very dry or moist atmosphere has the same effect. The former takes away caloric by an absorption of the perspirable matter—and the latter, still more, by evaporation from the surface.

Bœrhaave has remarked, that no animal could live long in a temperature much higher than its own. But this is not so, as has been demonstrated by Tillet, Fordyce, and Blagden. The latter two of these intrepid experimentalists, were shut up for a time in an apartment heated to 212°, and even to 260°—and the former exposed himself to a temperature of 250° for fifteen minutes, and to one of 285° for ten minutes, without much, if at all, increasing the heat of their bodies.

Now it is really curious to inquire into the resources of nature in such an emergency. I have already noticed some of the causes by which animal temperature is lowered. To a more extended operation of these, the chemical physiologists have resorted for an explanation. Evaporation from the surface is held by them to be the chief means in the reduction of excessive heat. But, though in the ordinary states of the body, it may have this effect, there seems to be great doubt whether it takes place under the circumstances of the case before us. It is a pretty well settled opinion, that perspiration is checked at a temperature very little above the natural standard,* and the tenor of our medical experience confirms the fact. By those who were subjected to the inordinate temperature in the experiments to which I have alluded, very opposite conclusions were made on the subject. It is reported by Blagden and Fordyce, that instead of sweating, their bodies proved, on account of their comparative coldness, condensers of the vapour of the room—while Delaroche asserts, that the perspiration poured down in streams from his person, so that a loss of weight of six or seven ounces, in eight or ten minutes, was sustained.†

* The experiments of Alexander make it 108° of Fahr., and subsequent ones, perhaps more accurate, reduce it to 102°.

† Nicholson's Journ. vol. xvii. p. 215, vol. xxxi. p. 341.

Which of these conflicting statements is true, I am not prepared to pronounce positively. They involve a point that can alone be decided by further experimental inquiries. Yet, I confess, that I can hardly resist the conviction of the correctness of the former, and that the effect is owing to a power totally independent of chemical agency. It is to be recollected, in support of this inference, that animals which perspire slightly, or not at all, do not less preserve their temperature*—and we have the very strong fact, perfectly well authenticated, of frogs and fishes doing the same in mineral springs† nearly at the boiling point.‡ It seems too, that were the reduction of temperature, under such circumstances, occasioned by evaporation, it ought to take place for a time at least as well in succulent dead matter, replete with juices, as in the living state. Contrary to this, however, we are told that the "fruits and meats carried into the heated apartments were almost instantly baked to a crust," while the individual subjected to a similar exposure suffered little inconvenience.§

An experiment, however, of Fordyce, which I have overlooked, is perhaps more conclusive than any thing hitherto mentioned, of the accuracy of his statement. He tells us, that while in a room heated to 130°, he had a Florence flask brought in, filled with water of the temperature of 100°, which immediately, on being wiped dry, became wet again, so as even to have streams of water poured down its sides—and which continued till the heat of the water within arose to 122°, when he left the room. It cannot be pretended that

* Blagden shut up a bitch in a room heated to 220°, and though after ten minutes she began to pant and hold out her tongue, not more so than dogs usually do in hot weather, and was so little affected, that throughout the whole time, which was half an hour, she showed signs of pleasure when approached, and on her release was perfectly brisk and lively. The natural temperature was here preserved, though there was no perspiration.—Trans. Royal Soc. p. 490, vol. for 1775.

† Sonnerat's Voyage to East Indies.

‡ Experiments made by Richerand confirm the above statement, vid. Phys. 24›

§ Richerand, Phys. p. 289.

evaporation took place in this case, and we have in it the most satisfactory evidence that at so high a temperature the process of condensation may proceed.

In closing this part of the discussion, I cannot forbear to cite the very interesting and instructive reflections with which Blagden winds up one of his communications on the subject. "A principal use," says he, "of all these facts is to explode the common theories of the generation of heat in animals. No attrition, no fermentation, or whatever else the mechanical and chemical physicians have devised, can explain a power capable of producing or destroying heat, just as the circumstances of the situation require. A power of such a nature, that it can only be referred to the principle of life itself, and probably exercised only in those parts of our bodies in which life seems peculiarly to reside. From these, with which no inconsiderable portion of the animal body is left unprovided, the generated heat may be readily communicated to every particle of inanimate matter that enters into our composition. This power of generating heat seems to attend life very universally. Not to mention other well known experiments, Mr. Hunter found a carp preserve a coat of fluid water round him long after all the rest of the water in the vessel had been congealed by a very strong freezing mixture.* And as for insects, Dr. Martine observed, that his thermometer, buried in the midst of a swarm of bees, rose to 97°. It seems extremely probable that vegetables, together with the many other vital powers which they possess, in common with animals, have something of this property of generating heat. I doubt if the sudden melting of snow which falls upon grass, whilst that on the adjoining gravel walk continues so many hours unthawed, can be adequately explained on any other supposition. Moist dead sticks are often found frozen quite hard, when in the same garden the tender growing twigs are not at all affected. And many herbaceous vegetables, of no great size, resist every winter degrees of cold which are found suffici-

* Essays Med. and Phil. p. 331.

ent to freeze large bodies of water. It may be proper to add, that after each of the above mentioned experiments of bearing high degrees of heat, we went out immediately into the open air, without any precaution, and experienced from it no bad effect. The languor and shaking of our hands soon went off, and we have not since suffered the least inconvenience."*

As another means which the chemical physiologists have assigned for keeping down animal heat, it is asserted that the consumption of oxygen is much less in a very high degree of temperature. Crawford, according to his own reports, has demonstrated this, and moreover shown, that under such circumstances, the usual change of arterial into venous blood does not happen, the one being nearly as florid as the other. It is hence inferred, no evolution of caloric takes place, and that it is highly probable the blood at the same time, acquires such a vast increased capacity, as to absorb all excess of heat. Let this be substantiated to the extent claimed, and doubtless the hypothesis would receive no slender support. But it turns out, that though there is some foundation for the statement of the reduced consumption of oxygen, at a high temperature, the difference is so immaterial as not at all to warrant the broad conclusion deduced from it. Delaroche has subsequently gone over with much care, the same field of inquiry. As the result of all he observed, he tells us, that the consumption of oxygen is rather less in warm blooded animals, at an elevated temperature, perhaps, as five to six. This, however, he admits was by no means uniform—in some experiments there being no difference whatever—and that, even the maximum is altogether disproportioned to the degree of heat developed under the circumstances.† Even this qualified admission is utterly contradicted by Spallanzani, so far as regards the cold blooded animals, to which his inquiries were restricted. Being numerous, I shall not detail his experiments. It may suffice to mention, that they incontestably prove, what

* Trans. Royal Soc. p. 122.

† Eclec. Rep. v. iv. p. 172.

indeed is confirmed by Delaroche, who repeated some of them, that heat increases most conspicuously the activity of respiration—and the expenditure of oxygen—in some instances, doubling and quadrupling the quantity, at the natural temperature.*

Complete in all its parts, as the chemical theory of animal heat has by many been considered, we find, from the preceding review of the grounds on which it is placed, that it is exceedingly vulnerable—so much so, indeed, that no one position it involves, is satisfactorily made out and substantiated. To the intrinsic imperfections in its composition, already indicated, I am now to add some objections, which I cannot help thinking altogether fatal to it. The first I shall notice are chiefly derived from Mr. Hunter, who was among the earliest to attack it. These go to prove the independence, to a certain extent, of animal temperature, both of respiration and the circulation. It is not required that I should circumstantially detail the evidence he adduces, consisting of cases of contusion of the head, of apoplexy, asphyxia, paralysis, and of the obliteration of large arteries, as in the operation of popliteal aneurism—where, though in the first series of instances, respiration was partially suspended, and in the second case, the circulation interrupted, animal temperature was preserved, and even heightened.†

Difficult as it may be to reconcile these facts with the chemical theory, much more repugnant to it, in my opinion, are some recent experiments of Mr. Brodie.‡ It is shown by this very ingenious physiologist, that in decapitated animals, the circulation may be restored by artificial respiration, and all the ordinary chemical phenomena induced, such as the consumption of oxygen, the corresponding production of carbonic acid, and the conversion of venous into arterial blood. Nevertheless, he found, instead of heat being generated under such circumstances, the animal cools

* Eclec. v. iv. p. 175.

† The same sort of evidence has lately been afforded by Mr. Earle. Med. Surg. Trans. vol. vii.

‡ Trans. Royal Soc. 1811 and 1812.

sooner than one of the same species in which artificial respiration is not performed—an effect he ascribes to the cold air thrown into the lungs.

An attempt has been made, without success, to impair the force of these experiments, by alleging that there is probably here, an increased exhalation, carrying off more than the usual degree of heat, and thus reducing the temperature of the animal. In support of this inference, an appeal is made to Le Gallois, who says, "that the lungs are always swollen and filled with a frothy fluid after decapitation, or division of the eighth pair of nerves." Granting it to be true—though the fact is not at all noticed by Brodie, the corollary seems exceedingly strained, and is, perhaps, wholly unwarrantable. Could an accumulation of mucus, or phlegm, in the cellular tissue of the lungs, lower the temperature of the body, as described, ought not the same effect to be experienced in the tussis senilis, asthma, and many other pulmonary affections, in which these organs are so heavily loaded and oppressed?

Nor does it appear, under any circumstances, that the quantity of caloric said to be developed by the oxygen which disappears in respiration, is equivalent to the quantity given out by the body. The fact of an exact correspondence in this respect I am aware was confidently asserted by Lavoisier and Delaplace. But the recent experiments of Dulong, which seem to have been made with great precision, lead to an opposite conclusion.* Taking the maximum of heat evolved under these circumstances, in a given time, it is less than that required to maintain the natural tempera-

* "He employed for that purpose a modification of Count Rumford's water calorimeter. The result was, that the quantity of caloric disengaged by the conversion of the oxygen into carbonic acid, is equal in carnivorous animals to between 49 and 55 parts in 100, of the heat disengaged by the whole body during the same interval of time—and in frugivorous animals, to betwixt 65 and 75 parts—and that the whole quantity of caloric disengaged by the formation of carbonic acid and water together, is equivalent to between 69 and 80 parts only. He thence concludes, that the animal heat is greater than can be accounted for by the fixing of oxygen during respiration, and therefore, that some other source of calorification must exist." *Journal de Physice.*

ture of the body in the most moderate climates, and totally insufficient in the lower latitudes. This being true with regard to the human subject, it applies more strongly to those animals, as the whale, in which the trachea and air cells are contracted and small, disproportioned to the general dimensions of their bodies, and which respiring only once in fifteen or twenty minutes, are surrounded too by the cold waters of the arctic regions.*

No chemical operation of any sort can possibly take place in the living state, and all views predicated of it must be fallacious. It belongs to vitality to counteract or suspend the play of chemical affinities or attractions, and when it is destroyed, fermentation or putrefaction ensues, which have no analogy to any of the processes of life.

Could any thing further be required to invalidate the chemical views on this subject, it might be supplied by the circumstance, that the blood in the right, is warmer than that in the left ventricle, so that it seems rather to be cooled, than heated, by the process of respiration, conformably to the notion of the ancient physiologists. Numerous facts equally irreconcileable to the pulmonic theory might be cited, were I disposed to indulge in such details. But I deem it superfluous, and shall be content with mentioning one more only, which I think subverts completely the whole fabric so ingeniously constructed.

Conformably to the established opinions of the day, the great changes incident to respiration take place in the terminations of the aortic system, and the blood, loaded with carbon, is returned by the veins to the lungs, by which it is thrown out in the state of carbonic acid. But these changes must be effected by the small or pulmonic circulation. The blood carried from the right side of the heart is dark, from the carbon which it holds, and that returned from the lungs is florid, or arterial, from the loss of this principle. It follows, therefore, according to the showing of the chemists themselves, that the blood of the aorta,

* Vid. Mr. Hunter's observations on the structure and economy of Whales. *Phil. Trans.* vol. 77.

and of course of the veins subservient to the same circulation, are wholly destitute of carbon.

On the whole, there is to my mind little to sustain any chemical view of the process of animal heat. The theories of this nature are deduced from false premises, and partake of one common defect. No direct or conclusive proof exists of respiration being a species of slow combustion, or that one particle of heat is evolved in the operation. The conversion of oxygen into carbonic acid, and of venous into arterial blood, may take place without such a result. What happens when venous blood is exposed to the atmosphere, out of the body? Do not all concur in the representation that it becomes florid, or arterial, that oxygen disappears, and carbonic acid formed? But who has asserted, much less proved, that any heat is disengaged? Experiments, which seem to me unexceptionable, show directly the contrary. To test this point, I drew blood in a bladder, into which was introduced a very sensible thermometer, and though as usual it became florid, and carbonic acid was formed, the mercury did not at all rise.

The closest coincidence may be traced in the results of this, and the experiments to which I have so recently alluded, where the same changes were effected in the blood by artificial respiration, and the temperature was even depressed.

In abandoning the chemical theory of animal heat, I confess that I leave the subject in great obscurity, having no satisfactory explanation of the process to substitute. But let us recollect that, according to one of the most philosophical of our writers, "an error detected is nearly as important as the establishment of a positive truth." The fact is, that we are still ignorant of the nature, as well as of the sources of heat generally, and before we can arrive at any definite conclusions, relative to the subject, these preliminary points must be determined. To limit the production of animal temperature to combustion in the lungs, or to the changes perpetually going on in the constituents of the body, is one of the many errors which the chemical phy-

siologists have committed. It is well known, that besides such sources of heat, there are several others, as condensation, percussion, friction, the electric and galvanic discharges, &c.

Now, is it an irrational conjecture, that there may be some undiscovered means peculiar to vitality, and that animal temperature is as much a function of life, as any appertaining to the economy of an animated being? The power possessed by animals, and in a less degree by vegetables, of preserving an equable temperature, by resisting the excesses of heat and cold, in every exposure, is alone sufficient, without again entering into the details of the subject, to confirm the hypothesis. It has lately been suggested, that heat is the result of a species of secretion, from the action of the nervous influence on the blood.* This, though ingeniously presented, is resisted by too many facts to be sustained, and proceeds from a narrow view of the subject. Whatever may be deduced in its favour, from the economy of the more perfect animals, it fails when applied to some of the inferior species, and vegetables, which, destitute of nerves, are still capable of evolving heat. Nor do the objections rest here. Cases are recorded where, from paralysis, nervous power seemed entirely destroyed, with a retention however in the limb, of its ordinary temperature.

With more plausibility, I think, it was previously conjectured, that "caloric entering the circulation with the chyle, passes in a combined state with the blood to the capillaries, which in performing the various processes of secretion, nutrition, exhalation, &c., give it out, and becoming sensible, constitutes animal temperature. Yet while corresponding with a larger number of the phenomena, there are difficulties also in the way of this hypothesis, impairing its validity, which might be pointed out, had I not already too long protracted the discussion.† It is with me a matter of some regret, that I cannot offer a more precise

* Philip Wilson.

† For a full account of this hypothesis, vid. Bichat's General Anatomy, vol. ii. p. 44, Heywood's translation.

theory on this curious subject than I have done. But in the present state of our knowledge it is perhaps impossible to do it. The facts well ascertained, relative to animal temperature, are very scanty, and with such slender materials, what could I hope to accomplish? Content, myself, therefore, with the more humble employment of clearing away a part of the rubbish, and preparing the site, I resign to the future labours of others, the erection of the edifice.

The ensuing paper will be read with interest, from the variety of curious and interesting matter which it contains. It comes from a sagacious and well informed practitioner, who observes with care and reports faithfully. Why will not some other of our friends capable of executing the undertaking, present us, from time to time, similar reports, in relation to their own experience? It is by such records, that we can only become conversant with the peculiar or modified diseases of our wide spread territories, and the modes of practice pursued in them with the greatest advantage.—Editor.

Art. III. *Reports on Diseases.* By Thomas Henderson, M. D. of George-Town. Read before the Medical Society of the District of Columbia.

REPORT FOR 1820, AND 1821.

Scrofula.

Though much has been written on this subject, much remains to be ascertained. The disease is truly proteiform. It may, however, be regarded in an interesting point of view for practical purposes, as an affection of the *external* and *internal* glandular system. When active in the latter, it is always, though not very speedily fatal, as in cases of scrofulous brain, lungs, liver and mesentery. When in the external glandular system, it may be considered as a fa-

vourable determination of the strumous habit, and care should be taken that it be not repelled by exposure to cold, or too indiscriminately curing scrofulous sores. Caution should be observed particularly in healing fistula in ano, where the strumous habit prevails. Who has not seen the intimate relation which subsists between that disease and phthisis pulmonalis? Bayle discovered a scrofulous tubercle at the bottom of a fistula.

In bad cases of external scrofula, I have given the saturated solution of lime in muriatic acid with very good effects. Nay, I have found it more serviceable than any or every other medicine. It is my practice to promote the the course of scrofulous glandular enlargements in the throat, groin, &c. at least, so far as frequently repeated fomentations with hot strong salt water will do. In the cephalea scrofulosa, which occurred in a woman, whose neck was scarred with the cicatrices of strumous suppurations in her youth, a seton procured entire relief. This woman had lost one eye from the disease. Flannel next the skin in winter, the feet kept very warm and dry, with abundant nutritious animal diet, are indispensably necessary in the scrofulous habit. Negroes with us, suffer very much with this disease—and it is in them I have seen so much benefit derived from attending to warmth in clothing, and ample nourishment. In illustration of the preceding remarks, I subjoin the outlines of some cases.

A young man, several members of whose family were consumptive, and who was subject to a dry cough, took a long ride on horseback—and on his return home, complained of deep seated pain about the left tuber ischii. After three weeks, there came an enlargement of the part, which very gradually increased, and at the end of two months, it pointed not far from the anus, and having every appearance of containing much fluid I made an incision: no pus, but much serum was discharged. Things continued for six weeks longer in this state, when the fistula healed, and a few days afterwards an enlargement of the left inguinal gland was perceived, attended with pain and redness:

the gland became very large, and after a tedious process it opened and discharged an oily fluid. These affections were left to nature, and the discharge continued some months, then ceasing, a cough supervened—when the groin again opening the cough subsided. By encouraging the discharge I find his health much improved, and he is advised, if the groin ceases to ooze, to have an issue in the arm.

In the year 1814, I was consulted by a young gentleman, who had a tumour near the anus. He was temperate—apparently of a sound constitution—not in any way predisposed to pulmonary disease—his parents living, and in advanced life. He had taken a long ride in cold weather. The local affection went on to suppuration, and he being restless and impatient under confinement, would not allow the abcess to granulate and heal. It filled again, required a second opening and became fistulous. After a slight operation it healed, and soon after, pulmonary symptoms came on, ending in death.

Heberden observes, that on the retreat of the outward symptoms of scrofula, which usually takes place about the age of puberty, the disease falls on the lungs, producing an incurable consumption. This remark is sanctioned by general experience, and is especially so, with regard to fistula in ano. Now, to what practical conclusion should this lead us?

1st, That scrofulous sores must be healed with great circumspection. I have more than once seen exertions made, very imprudently, to cure the hip joint disease—which being done, the strumous disease appeared in the lungs.

2d, The connexion between fistula in ano, and scrofulous consumption, is carefully to be remembered. We should be extremely cautious at all times, in operating for this disease, to produce a healing of the sinus. But on a general view of the patient's predisposition, either hereditary or acquired, finding the scrofulous skin, eye, lip, the purely white teeth, or in early life, the tendency to chronic enlargements of the glands about the throat, with the conformation of the chest favourable to consumption, we ought to decline to do it al-

together. This would be my decision, from a knowledge how tolerable in many instances, an existence even to old age, may be passed with a fistula, if the patient is ordinarily discreet in his habits.

The late professor Dorsey says, "I have met with so many cases of fistula in pulmonary consumption, that I am disposed to think it symptomatic of that disease. In all the instances I have met with, the patient has died soon after the healing of the fistula."

By Mr. Charles Bell we are told, that "an abscess of a very bad kind, is peculiarly apt to form by the side of the anus in consumptive people. If the physician gives up his patient into the surgeon's hand to be cured for fistula when there is a phthisical cough, the latter may expect little credit to result from the operation."

Copeland says, "fistula is frequently met with in persons who have pulmonary symptoms. The propriety of performing the operation under such circumstances, must depend on the degree of this last disease. The discharge has sometimes been considered favourable to the lungs."

I have now a case under my care of hemorrhoidal affection, very troublesome and painful, having an evident connexion with a cough. Pulmonary symptoms to an alarming extent were present until the hemorrhoids came on, since which, the lungs have been relieved. But whenever the piles are entirely cured, there is a sensibly irritation of the lungs.

Cholera Infantum.

During the present season this disease was more dysenteric in its character, and more frequently attended with aphthæ, than usual. The griping, tenesmus, and bloody stools, rendered the practice with calomel and castor oil, frequently repeated, the most effectual.

I saw one case of cholera with symptoms peculiarly striking. A fat hearty boy, while cutting his teeth, was seized with vomiting and purging. The disease occurred in the country, and after some days the child was brought to town.

It commenced with coldness of the feet, legs, knees, hands, arms, and nose—stomach irritable—pulse weak and frequent—the abdomen hot. The coldness of the extremities proceeded to a degree that I never saw equalled in the living body, and, *pari passu*, was the heat of the abdomen. This state continued until, by free purging with calomel and Epsom salts, frictions to the cold parts with hot spirit, and wrapping them in hot flannel, and also by the warm brine bath, with active friction on the abdomen while in the bath, the congestion was removed and the excitement equalized. The exhibition of internal stimulants in this state might have increased and rendered fatal the congestion in the abdominal viscera. Each paroxysm continued several hours.

My experience this year more fully impresses me with the conviction of the intimate connexion between the irritation of dentition and the symptoms of cholera, as cause and effect. How few children have cholera who are not actively engaged in dentition? How powerful an irritant is this process to the infantile system? Hence how necessary it is to remove it by free incisions down to the teeth. We learn from the late Dr. Rush, that he has seen the little patients revive and cheer up as soon as they inhale the pure air of the country. The same remark may be applied to the instantaneous relief afforded on the gums being freed from the distention of the tooth.

Not the least distressing and fatal complications of disease is that of cholera and pertussis. The effect is most frequently a determination to the head, which once taking place may be considered inevitably mortal.

Pertussis.

The practice in this disease seems to be well settled. Emetics, followed by an occasional dose of calomel and castor oil, are sufficient for the generality of cases. But when any thing like pneumonic inflammation supervenes, the lancet and blisters are essential. I am confident, that during the present season, I saved the lives of three children, by copious bleeding and blistering. As a tribute of

respect to a distinguished practitioner of medicine, I would observe that much is due to Dr. Watt, of Glasgow, for his resolution in investigating the state of the lungs after death in his two children, to whom this disease proved fatal, as well as for his researches into the pathology, and the determination of the importance of venesection in pertussis.

Heberden makes a remark on the pertussis of adults, which is somewhat curious and exceedingly interesting. He says that a child has notice of the approach of a fit, so as to run to its mother or nurse before it begins—while adults, overpowered, as it were, at once on the accession of it, fall down instantly as in an apoplexy, though they very soon come to themselves. This is so distinguishing a symptom of the disease in those who are grown up, that if they have not before had it, and have recently been exposed to the atmosphere of pertussis, it may remove all doubt as to the nature of the affection.

Dysentery.

I have already observed that cholera infantum was more than usually attended with dysenteric symptoms. The state of the weather, or atmosphere, or whatever it may be, seemed to predispose much to looseness of the bowels. This was very troublesome in many cases of bilious fever, and in some led to a fatal termination. On this, however, as well as the livery which it caused the typhus, typhoid, and common fever to wear, I shall have occasion to remark hereafter. The proportion of dysenteric patients was much greater during this than last year—and measles were accompanied with very lax bowels.

In one case of dysentery, which resisted the ordinary means, I gave relief by the adoption of a practice which originated in India, and has been used in some bad cases in London, with success. The prescription is a scruple of ipecacuanha, and twenty-five drops of laudanum, to be taken at one dose. It produced nausea, and very gentle vomiting, with greater relief for several hours than had been obtained by any other preparation or combination of opium

which I had previously used, and led to the cure of the disease.

Generally, the practice in this disease may be considered as so well settled, that it is only in extraordinary instances that difficulty occurs. In such, it is well for the medical man to be prepared for every emergency. While the subject is under review, I will mention, that some years ago, in a case of chronic dysentery, attended with an obstinate and excessively distressing singultus, of nine days continuance, I effected a cure, by giving ten grains of carbonate of ammonia, and one grain of opium, every three hours.

In two cases of dysentery I found a symptom which is very troublesome, and in one of which the disease terminated fatally. The symptom I allude to is an immediate disposition to stool after swallowing any food, solid or liquid. The stomach here, I presume to be affected as is the internal coat of the bowels—so that an impression from morbid continued sympathy, runs at once through the whole intestinal canal, and which is rendered more probable by the continuity of the mucous membrane from the mouth to the anus. This symptom I have seen only in two cases. All the astringents, anodynes, external irritants, and stimulating frictions, were used with no effect in one, and the other terminated favourably under the use of Dover's powder, and well regulated farinaceous and milk diet.

Bilious Fevers.

The present year is distinguished by three epidemics, measles, pertussis, and influenza—and to these may be added intermittent fevers. For many years past the last have been very rare, except in certain localities. Last year the proportion of intermittents to the other forms of fever was, in my practice, one to eleven—and this year as one to four. In the year 1819, typhus, to other forms of fever, was as one in eighteen—and this year one in fourteen cases. No material difference exists in the meteorological records of the two seasons.

Little need be said of the treatment of bilious remitting fever. The early and sometimes repeated use of emetics and tartarized antimony, should always be preferred—calomel in purgative and smaller doses—blistering—with antimonials, and tepid bathing to relax the system, constitute the means of cure. Blood-letting I have very seldom found necessary.

Every form of fever prevailed this fall. In the beginning the intermitting, remitting, with and without chills at the outset—the continued bilious—and, as the weather became cooler, typhous and typhoid fevers appeared. The danger in the cases of typhus was derived from the supervening of local acute and sub-acute (chiefly the latter) inflammation. This occurred in the head, chest, and abdomen. Typhus was, almost in every case, ushered in with violent pains in the legs. The sub-acute inflammation of the lungs I met with most frequently as the influenza prevailed, but the disease, though obstinate, uniformly yielded. In one case of determination to the head, and another to the intestines, producing inflammation, the result was fatal, and death speedily occurred. An emetic, calomel as a purgative, blisters, calomel and ipecacuanha, Dover's powder, formed the treatment I pursued in the typhous cases. Bringing the system under the specific effect of mercury, aided by bark and wine, constituted the safety of the patient in the latter stages. By the specific effect of mercury I do not mean severe salivation, but an exhibition of it so as to perceive it in the breath, gums, and *pulse*. A severe salivation, however, where it took place, interfered only with the comfort, never with the security of the patient.

The dysenteric state, incident to many cases of this fever, called for purging and Dover's powder. The use of purgatives was absolutely necessary, to remove the irritation which kept up the looseness and pain in the bowels. In the state of intestinal fever now under consideration, the most palatable and useful diet consisted of equal parts of well made coffee and boiled milk.

The red and glassy state of the tongue was very prevalent in our fevers this fall.

My remarks have been, and will continue to be, intentionally desultory. It would be impertinent to present to a society of practitioners a regular description of bilious fevers. They are, generally speaking, very uniform diseases—varying chiefly in the features of remission and intermission, on the causes of which differences no satisfactory information has been obtained. What occasions the disproportion in the number of intermittents in two seasons, such as has been given above, while the seasons were generally alike, has not even been surmised.

One remark may be subjoined. Three of the epidemics of this year were diseases affecting the mucous membranes—pertussis and influenza that of the bronchiæ, and dysentery the lining of the intestines. *Aphthæ* lining the mouth will be found troublesome whenever dysentery prevails, arising, probably, from the irritating miasm affecting, with peculiar violence, that portion of the mucous covering which is particularly prone to ulceration.

1821.

Tetanus.

A case of this disease occurred, and terminated fatally, in a young man, by trade a butcher. The cause of the disease was a wound on the finger-joint of the left hand, by a ragged piece of bone. The period of attack, after the infliction of the injury, was fifteen days, and the duration of the disease from the supervention of opisthotonos, nine days. For a week, previous to the spine, neck, and abdomen being affected, he went about with the jaw rigid, unconscious of his condition.

The treatment was adopted on the principle of bringing four remedies to bear on the disease, viz. opium, mercury,

wine, and caustic. It was believed that there would be no incompatibility in the impressions or effects of these remedies. The patient had been freely bled before I was called in. I commenced the treatment with twenty-five drops of laudanum every second hour, rapidly increasing it to two hundred drops every fourth hour—in addition to which a strong opiate friction was made to the abdomen. Calomel was at first given, so as to purge freely, afterwards in smaller doses, and the strongest mercurial ointment was rubbed on the abdomen. Wine was exhibited as freely as I could induce him to take it—but as he had a great aversion to it, not enough could be given him. Next day I applied the potass. é. calc.—which was effectually done on the cervical and some of the dorsal vertebræ—and I am convinced that the pain of the application, and subsequent burning, increased materially the spasms. The practice is old. Cauterizing has been used for half a century in the West Indies, by French practitioners, without much success. But we are told that the result has been otherwise in some cases in this country. Nevertheless, I cannot bring my mind to admit that it promises benefit in tetanus—and I am particularly averse to irritating the wound in any way after the disease has taken place. In the present case I found, on examination, a spicula of bone, with a serrated edge, in the wound, which, as an irritant, common sense and all authority would teach me to remove. On what principle, then, am I to take out a bone, or nail, or splinter, and insert a piece of the vegetable caustic, or cantharides. Is there any thing specifically distinctive in the impresions of the irritants—or is it likely that exciting suppuration, with a discharge of a few drops of pus, would make any salutary impression on such a disease as confirmed opisthotonos?

I used neither the warm nor cold bath. Whenever an attempt was made to move the patient for any purpose, the spasms were so *horribly* increased, as to deter me from the attempt. Even in the administration of an enema, so violent were the spasms, that at last he positively refused to submit to the remedy.

With regard to purgatives, I should use them just to remove irritation from the bowels. Dissections, so far as I am informed, give no reason to believe that organic or functional disorder exists in the abdominal viscera. And I ask, whether Dr. Hamilton's arguments, on the use of purgatives in tetanus, are not sheer sophistry—his reasoning much more overstrained than the muscles of his patients in the cases he details?

My remarks are applicable only to opisthotonos from wounds, or traumatic tetanus, as it has been called. There is a tetanic, spasmodic affection produced by cold, in which fever and symptoms of visceral derangement exist. But this is not the original or principal disorder, and as the fever is removed by venesection and purging, the spasms might certainly be expected to disappear, and especially by the aid of the warm bath, and other relaxing and diaphoretic means. Nor is it, so far as I can judge from authority, difficult to distinguish this case from traumatic tetanus.

In future I shall place reliance for the cure of symptomatic tetanus, on opium and mercury, exhibiting the former to an extent bounded only by stupor and other full effects. Instead of commencing with a small dose, I would recommend Dr. Morrison's plan of giving one hundred drops, increasing one-third every three hours. This is advised in that standard work, Johnson on Tropical Climates. Professor Gibson, in a conversation with him on this subject, confirmed me in this determination as to opium, by some very striking results in his practice. Whatever increases muscular and nervous irritation I will remove—whatever allays it I will use—be these remedies what they may.

In the second number of Cooper and Travers' Surgical Essays, Mr. Cooper observes, that if in traumatic tetanus he has seen any remedy do good, it is opium and mercury. Those cases which terminated favourably in his practice, did so under such treatment. He says, there is a chronic tetanus where recovery is spontaneous—and it is from such cases, I believe, some remedies derive their celebrity.

On the subject of tetanus, I think the remarks of Mosely, in his treatise on tropical climates, very well worth the attention of every practitioner.

Spontaneous Evolution of the Fœtus.

The attention of practitioners has lately been called to this effort of nature, by Dr. Douglass, and still more recently by Mr. Gooch. It is a very interesting subject, and it may not be useless to report the experience which I have on it, with such reflections as may be suggested.

This evolution is observed to take place almost exclusively in presentations of the superior extremities.

Case 1.—I was called to see a healthy young woman in labour with her third child. The left arm and shoulder presented, and the pains were very strong. In endeavouring to turn the child, though I succeeded in introducing my hand into the uterus, with all the exertions I thought it prudent to use, I could not carry it up high enough to reach the knee or foot. The uterine action became irresistible, and I withdrew the hand, requesting the midwife in attendance to await the result of the pains, and as soon as they abated to let me know. Just as I was stepping out of the door of the room, I was called to see a great change, and on examination, the whole of the child's lower extremities and body were delivered, and in two more pains the head came. The child was dead, but the mother did well.

Case 2.—The late Dr. Muschett, of Dumfries, Virginia, a physician of acute mind and considerable accomplishment, gave me the following history. He was called to visit a case of arm presentation, where the limb had been removed by an ignorant practitioner. He found the child still crossed, and while preparing to turn it, to his great surprise, this suddenly took place spontaneously—the lower extremities turning down, and the labour was soon after completed by the natural efforts. The woman did well.

Case 3.—Dr. N. W. Worthington has given me the following case He was called in consultation, and found the woman in labour, with the arm, shoulder and umbilical

chord presenting. He succeeded in reaching a foot, which he brought down and secured with a fillet. Notwithstanding this, the action of the uterus turned the child—the presentation became a natural one, and the head was delivered as in ordinary labour. The arm did not return into the uterus.

This process of spontaneous evolution is interesting as a matter of curiosity, exhibiting one of the various resources nature develops for the accomplishment of her purposes, and is not without practical importance. In my opinion, Dr. Hamilton gives a very happy explanation of the cause of spontaneous evolution. The contractile force, says he, of the uterus, may or may not act on the presenting part in such cases: "if it does, then the turning will not take place: but, if from any circumstance, this expulsive power should be directed to any other part of the child, then that part will be protruded, and the arm withdrawn." Perhaps he is not correct in saying the arm will be withdrawn.

It has become a question, whether any rule of practice is to be drawn from this spontaneous evolution. This involves the course to be pursued in arm cases. By common consent, in presentations of the superior extremities, it is decided, that the child is to be turned if possible. But the utmost patience, prudence and judgment should be exercised, in ascertaining whether this be practicable. Where a fillet can be applied to a foot or knee, it renders the delivery of the child certain, and by cautiously persevering in gradually carrying the hand forward almost every case may be turned. That, however, cases sometimes occur, where the practitioner cannot safely overcome uterine action, and force his hand up, or even allow it to remain in the uterus, I know from having experienced it in one of those above detailed. What then is to be done? We are simply to wait, not for spontaneous evolution, but for the pains to cease, or until the action of the uterus is so far exhausted as to admit the introduction of the hand, and the accomplishment of *artificial* turning. That this passive or exhausted state of the uterus takes place in arm presentations

I have learned from repeated experience, and by waiting for it, have ultimately turned with ease.

In arm cases, a difficulty is said to exist in the swelling of the protruded extremity from the contraction of the uterus on the parts above, and the determination of fluids to the arm. This difficulty I have never seen. Whether Smellie is justified in considering this as a case requiring amputation of the arm, I am not prepared to say. On this point, I refer with particular satisfaction to a paper in the 11th vol. p. 30, of the New England Journal, written by my friend Professor Channing, of Boston, where the needlessness of amputating the arm to facilitate delivery is shown—and where in turning the child in a fully presenting arm case, the *artificial* evolution was performed *without the arm being at all* returned into the cavity of the uterus or even into the vagina.

Cynanche Tonsillaris Biliosa.

I have for the first time met with a disease, which, from its general prevalence and complication, I have named as above. By way of introducing what little I have to say on this subject, I shall give the case of a Miss ——, ætat twenty-two.

May 29.—Was taken this morning with pains in the limbs, head and back—sore throat—tonsils enlarged and ulcerated—pulse frequent and tense—skin moist—tongue white and loaded with fur. In the evening headach increased—bowels costive—nausea.

> Ordered bleeding to ℥ xiv.
> ℞ Merc. dulc. grs. x.—stat. sumend.
> A gargle of sage tea—honey and vinegar.
> Diluents.

30th.—8 A. M. Passed a restless night—delirious—headach—throat painful—fever not so high—skin cool and moist—pulse frequent—no operation from the bowels.

> Directed the senna infusion with Epsom salts—
> Gargle, with lemonade and sage tea—
> Cold vinegar to the forehead.

31st.—8 A. M. The purgative infusion operated copiously—had a paroxysm of fever yesterday afternoon, not so high as before—headach continues—pulse frequent, though not tense—skin moist—tongue covered with yellow fur—tonsils enlarged and encrusted with a very thick layer of yellow tenacious fur or slough.

Directed the saline draught, with vin. antimon. and as a gargle, acid. muriat. cum infus. ros.

June 1st.—8 A. M. Convalescent—throat better, but unusually coated with slough.—Continue the gargle.

The above case is a good specimen of the disease as I saw it. It attacked persons of all ages—though in my practice, chiefly females and children. It commenced and continued its course very much like an ordinary bilious remitting fever. Generally chills, with a pain in the back and limbs, ushered in the disease, with sore throat—which sometimes was complained of a day or two preceding the chill. The tonsils were considerably enlarged and slightly reddened. In some cases there were distinct ulcerations, and in all a secretion of yellow mucus, which adhered for three or four days, and separated in a thick layer. This layer was in some instances to be detached with the handle of a spoon.

In a lady advanced in life, the tonsils and fauces were of a very dark colour, which was the only instance where any thing like a putrid tendency appeared. In a child of ten years old, there were four distinct attacks of the disease in the course of three months. This repeated irritation and inflammation, left a permanent enlargement of the tonsils. In no one instance was there any attendant eruption, nor was the lining membrane of the mouth and fauces affected with redness, as in scarlatina, two cases of which, only occurred in this quarter. In the city of Washington, a few cases of putrid sore throat appeared, but I have not heard whether they were accompanied with eruption. I observed considerable fœtor in the breath of some of my patients. The fever was sometimes intermittent, and considerable vomit-

ing attended. Generally, the disease completed its course in five days. In the treatment, I commenced with an emetic, then purging, and next antimonials, with acidulated gargles. Blood-letting I used in one case only.

Of swelling of the tonsils, and ulceration of the throat, sporadic cases occur at all seasons. But having never seen so general and distinctly marked a disease of the kind, I have thought it my duty in making up a quarterly report, to give a brief account of it.

Curved Spine.

I met with one interesting case of this disease. The boy was four years of age—had been out of health for several months with disordered stomach, and occasionally irregular bowels and fever. He was observed not to be so active, and soon to trip and fall by one foot being involuntarily thrown before the other. He gradually lost the use of his lower limbs, which became rather full than flaccid, with attenuated muscles. On examination, a curvature was discovered in two of the lowest dorsal vertebræ, and I recommended the use of caustic issues, which was assented to by the parents. But, an older medical friend seeing the child, advised " those nasty things" setons, as Mr. Pott was wont to term them. They were accordingly introduced on each side of the spine near the curve or caries. The child was much terrified by the use of cutting instruments. The day after he was taken with high fever, costiveness and irritation —to relieve which, a dose of calomel was given, followed by Epsom salts. The second day the fever continued and increased, and at night he became exceedingly restless, anxious and oppressed. The same medical friend took out the setons as a probable cause of irritation—and administered *fifteen drops of laudanum.* Three hours after this I saw him in the agonies of death, with convulsions and stupor, and in half an hour he expired. Need I observe, that the warm bath—cold to the head—a repetition of calomel—general or local blood-letting—enemata—and febrifuge

draughts, should have taken the place of the fifteen drops of laudanum.

Spina Bifida.

Miss M——, aged 18 years, has repeatedly been my patient for hysteria and head-ache, with pain in the back. Her complexion is sallow—menstruation very irregular and sparing, and she is swelled. I was about to prescribe aloetic medicines, with steel, when my attention was called to a tumour on the right buttock.

This tumour is congenital. At her birth, it was as large as a pullet's egg, contained a fluid, and was pronounced by Dr. May, sen. to be a case of spina bifida. From the experience and judgment of Dr. May, there can be no doubt that such was the fact. It has continually increased until now, and a crucial measurement would make it about six inches, and prominent, three to four inches. It contains a fluid, apparently in a cyst of very thin consistency—is painful on pressure, and seems to have a connexion with the sacrum or coccygis, which are mal-formed and turned towards the right side. The pain in the back and head is excessive, followed sometimes by epileptic convulsions. Her general health is bad, although she frequently walks about very much, and actively. With the advice of Professor Gibson, and other able surgeons, who examined the tumour, it was thought best to leave the case to its natural tendencies.

Bilious Fever.

The early part of the season was uncommonly cool, with frequent rains. This cool weather had the effect to prevent the usual frequency of *cholera infantum*; and as far as my information extends, the deaths were very few. The same remarks apply to *dysentery*, which was mild and manageable. In the sphere of my practice, *bilious pleurisy* was more frequent, and severe cases of it occurred in the second quarter of the year. *Bilious colic* was also common, and many cases of the fever commenced with it. Diarrhœa was not frequent, and generally mild.

Throughout the country the season proved a very fatal one. Few places escaped. Exceptions occurred in the cities of Charleston, Savannah, and New Orleans, all of which were almost entirely spared the ravages of malignant fevers. But some of the northern towns were scourged, and the interior of the country was, as already intimated, much afflicted with bilious fevers. My attention was called to a remarkable fact in relation to the partial mortality of fever in the District of Columbia, as exemplified by the following statement. During the months of August, September, and October, it is well known that in Alexandria, a neighbouring city, a malignant fever occurred which carried off many of the inhabitants. The bills of mortality of Washington, state the deaths from the several forms of fever, in those three months, at 94, out of a population of between 12 and 13,000; while in George Town, which adjoins it, out of a population of between 7 and 8,000, there were not more than *eleven* deaths from the autumnal and bilious fevers—I am nearer the truth when I say only *nine* died. I speak positively on the subject as regards George Town, having taken the best and most certain means of ascertaining the fact.

I conceive it my duty, in justice to the pretensions of of George Town to the character of being unusually and uniformly healthy, to state, that Dr. Huntt, of Washington, for whom I have a perfect personal regard, is entirely in error when he states that our town was "peculiarly healthy this season." The peculiarity was in the *exemption from mortality* in the cases of fever, not in our freedom from cases of fever numerically considered. Nor is he more correct when he says, that "heretofore George Town could never boast of as much exemption from disease as Washington." I have no doubt that the error is, on the part of my friend, entirely unintentional, arising from want of accurate information. It is from the assurances of our oldest and most respectable medical men, from information derived from our oldest inhabitants, from personal knowledge for nearly seven years, and from the obvious locality of the two cities, I can state, that, as to frequency of cases, mild-

ness of character in summer and autumnal diseases, and remoteness from the *fons et origo* of bilious diseases, George Town possesses a superiority over Washington which *always has*, and so far as judgment can be formed, *always will*, constitute the more healthy town.

I shall not pretend to give any detailed account of the different forms of intermittent, remittent, and continued fevers which prevailed. The most troublesome symptom I saw this season, was the vomiting which attended the intermittents, commencing an hour before the chill, and continuing, in some instances, for three or four hours, attended with profuse sweating. After evacuations, opiates, effectually checked it.

I must again be permitted to speak most decidedly as to the effect of mercurial treatment, in cases where the disease, not yielding in a few days to ordinary practice, continued with threatened violence to the system. I know that it is said, where mercurial action cures, the cases are such as would have terminated in recovery without it. When I state, that in the worst cases of bilious fever, after puking and purging freely, and using other means, I find no amendment about the sixth day, and the disease running on with a prospect of local disorganization—in this state of things, instead of relying on the already tried plan, I give two or three grains of calomel every three hours, with or without the antimonial powder, with the particular object of producing the mercurial impression—and as soon as this is attained, I find the fever cut short, and the patient recovering almost simultaneously, with *mercurial fetor in the breath* and an *influence on the pulse*. It appears to me, under these circumstances, a palpable absurdity to say, as I have heard it said, that these are cases that would have done as well without the mercury. I state facts, and leave the anti-mercurialists to their contingencies.

I have never seen this mercurial practice more remarkably efficacious than in the case of a woman, with a violent bilious fever, in the fifth month of pregnancy, who miscar-

ried on the thirteenth day. Finding the fever continue unaffected by the freest evacuation by puking, purging, and the use of antimonial diaphoretics, I commenced, on the seventh day, with calomel in small doses, three grains every four hours, with occasional laxatives. The fœtus was thrown off on the thirteenth day, and she sunk into a stupor, for which she was extensively blistered, with so little effect, however, that she seemed to all around her as certainly to die. On calling early one morning, I found her pulse more regular, and to my great satisfaction, the mercurial fetor in the breath, which went on to salivation, and she finally recovered entirely.

I give the above case as one, excepting the attending pregnancy, illustrative of very many I have met with in my practice.

The eleventh number of this Journal contains a brief Review of a recent work by professor Warren, of Boston, on the Nervous System, in which the subject of phrenology is incidentally touched. The views presented in the work itself, as well as in the critical notice of it, are somewhat disparaging to this new science. In a city, claiming a most numerous and respectable association for the special cultivation and advancement of phrenology, such an attack could hardly be expected to be silently endured. In the ensuing Lecture, delivered before the society, by one of its most ingenious and learned members, we have, accordingly, its replication and its defence. We take no part in the discussion of this subject, and have really paid so little attention to it, that we are scarcely competent to express any opinion concerning it. As an editor, however, our maxim is "audi alteram partem," in all cases where the controversy is conducted with courtesy. On this principle we cheerfully admit the article, and believe that it will reward the most careful perusal.—Editor.

ART. IV. ***Comments on some of the illustrations derived by Phrenology from Comparative Anatomy—with reference to a late Review of Dr. Warren's work on the Nervous System.—Delivered, as a Lecture, before the Phrenological Society of Philadelphia.*** **By B. H. COATES, M. D.**

IN the course of our inquiries into the connexion between the different developments of the powers of mind and those of the corporeal organization, we are frequently assailed with attempts to prove that inquiries of this kind are altogether fruitless, and to possess the public mind with the idea that the statements of facts made in the works on phrenology are not to be depended upon. This sweeping proscription, often arising from respectable and influential sources, paralyzes inquiry and diminishes or destroys the liberty of discussion. It therefore becomes necessary, occasionally, to reply to publications of this nature, and I am persuaded you will not deem the time spent amiss, if I occupy you this evening with a review of some points which have been lately contested.

In an uncommonly elegant little treatise lately published by professor John C. Warren, of Boston, there are several views on these subjects contrary to those to which we have been led. These, as they are not carried to any great extent, and more particularly as they are expressed in the true scientific tone and temper, would, of themselves, need no other discussion or reply than those inquiries into nature and books, to which the essay in question is calculated to lead the mind. Under the impression that such references to fact could ultimately lead only to truth, they might have been safely left to themselves, were it not for the peculiar situation of phrenology here at the present time, and the use which has been made of Dr. Warren's work to oppose it. To this, then, and to a review in the last number of the Philadelphia Journal of the Medical and Physical Sciences, in which unfavourable conclusions are rather hastily drawn from Dr. W.'s statements, I shall take the liberty, in the course of this lecture, particularly to allude.

It is one of the oldest, as it is one of the most obvious

remarks in physiology, that the pre-eminence of man, and the superior animals over others, in point of intellect, is accompanied, as a general rule, by a proportionate size of that part of the head which contains the brain. We find it a remark bearing antiquity nearly as high as the first prosecution of anatomy and physiology. Of late years, however, it has been called in question, by persons who deemed it to interfere with their previously conceived opinions upon the nature of the soul and of human superiority; these writers forgetting that the only mode of discovering truth in nature is by investigating all the facts upon which it is founded, and that it is not for the finite powers we possess to prescribe, a priori, in what manner the works of God shall be found to be constructed. It is much more suitable, according to the opinion of one who was at the same time a naturalist, a clergyman, and a pleasing writer, to employ ourselves in the investigation of the created world as it actually exists.

It is obvious that many of these start with the previous persuasion that the immaterial soul constitutes the *only* difference, as regards mental powers, between man and animals. All other modes of explaining that difference, though derived from an investigation of nature, they reject; because, in their eyes, such explanations lead to materialism. I shall consider you convinced, that no inference in favour of materialism is to be drawn from the principles of phrenology. This forms a proper subject for a separate discussion. It is sufficient for our present purpose to say, that all reasoning from consequences upon the subject is overthrown the moment it is found to be inconsistent with observed fact.

In looking over the tables which have been constructed to elucidate this question, we are struck with what is acknowledged on all hands to be at least the *general* rule. Throughout, by far the greater number of animals, whose brains have been examined, and of whose sagacity and force of propensity we can form an estimate, the rule holds to a

remarkable extent; though particular exceptions occur, which are considered by the opposers of this principle as proving it fallacious. I shall take the liberty of reading you some extracts from the table furnished by Cuvier, of the relative size of the brain in different animals.*

* The following is the celebrated table of Cuvier, from which extracts have been made by Dr. Warren, but which has not been republished entire, to my knowledge, in any American work.

The proportion of the weight of the brain, is to that of the whole body, in

Man, as old or young, as	1:22,25,30,35
Generally averaging	30
Orangs.	
Gibbon	1:42
Sapajous.	
Saïmiri	22
Saï	25
Ouïstiti	28
Coaita	41
Guenons, or Monkeys with long feeble tails.	
Young Malbrook	24
Callitriche and Patas	41
Mone	44
Mangabey	48
Baboons and Macaques.	
Macaque	96
Magot	105
Baboon	104
Makis.	
Young Macauco	61
Vari	84
Bats.	
Noctula	96
Plantigrades.	
Mole	36
Bear	265
Hedge-hog	168
Carnivora.	
Dog	47, 50, 57, 154, 161, 305
Fox	205
Cat	82, 94, 156
Panther	247
Martin	365
Ferret	138

Rodentia.	
Beaver	1:290
Hare	228
Rabbit	140, 152
Ondatra	124
Rat	76
Mouse	43
Field-mouse	31
Pachydermata.	
Elephant	500
Wild boar	672
Common boar	412, 512
Siam hog	451
Ruminants.	
Stag	290
Yellow roe-buck (chevreuil jaune)	94
Sheep	192, 351
Ox	860
Calf	219
Solipeda.	
Horse	400
Ass	254
Cetacea.	
Dolphin	25, 36, 66, 102
Porpoise	93
Birds.	
Eagle	160
Falcon	102
Sparrow	25
Canary	14
Tarin	23
Pincon	28
Redbreast	32
Blackbird	68
Cock (of domestic fowls)	25
Duck	157

It might, perhaps, be sufficient to say, that in this most obscure and unexplored part of an incomplete and uncertain science, namely, physiology, the principle so generally acknowledged, of admitting exceptions, should be peculiarly claimed. In none is there more probability of meeting cases, either not understood or affected by extraneous and unknown causes. Here, as in all branches of physiological and medical science, that theory is the best, which agrees with the greatest number of observations; nor can we, even when acknowledged to be in the right track, expect for a long time, to be able to explain the inferences to be drawn from the structure of a variety of animals, of whose habits we know so little, and of whose propensities we are almost totally ignorant.

It may be also said, that remarkable anomalies, such as that of the elephant, can be explained by means of a phrenological principle, to an extent which no other method can reach. It is thus we are taught that an animal may be intellectual, and yet because it is at the same devoid of violent propensities, and the corresponding organs are small, the size of the whole brain may be far from considerable. The bulk of Gall's organs of intellect, even in man, is a very inconsiderable portion of the whole brain; and moralists, unfortunately, have often occasion to remark that the force of reason in

Goose	1:360	*Fishes.*	
		Shark (requin)	1:2496
Reptiles.		Sea-dog	1344
Land tortoise	2240	Tunny (thon)	37440
Sea tortoise	5688	Pike	1305
Collared snake (couleuvre a collier)	792	Carp	560
Frog	172	Silurus glanis, the seat fish, a congenes of our catfish	1827

In a sajon from South America, the proportion of 1 to 11 was asserted to have been found; which appeared incredible to Haller, from whom it is quoted; but Sœmmering informs us that in the mature fœtus of the simia cynamolgus, he has seen it reach that of 1 to 8. It is probable that the specimen mentioned by Haller was also young.

The variable proportions generally arise from difference in fatness, as also of age.

From the above table of 56 animals, Dr. Warren has given us 32; and these containing the principal exceptions; a method not adapted to give a perfectly fair impression of the general fact.

contention with the passions, is correspondingly weak. In creatures, then, of enormous strength and size, if the same relative propensity to anger, cruelty, &c. existed; or if, as I shall have again have occasion to observe, they had propensities to mutual assistance and friendship, and to extraordinary cunning and caution, the parts of the world which they frequent would not be habitable by animals of other species, or by man. Accordingly, in the tiger, of which a very fine specimen is in your collection, the mass of these propensities, particularly the destructive one, is indicated in a much less proportionate degree, than in a small animal of a similar species, the common cat. Although the ferocity, strength, activity, and weapons of the tiger, render it so destructive a creature, yet we do not hear of its lying continually in wait for its prey, and seeking it out and killing it for mere amusement, as is done by the cat. Its ravages are principally confined to what it requires for food, and to those men and animals who are so unfortunate as to cross its path or invade its jungles.

Hence, in the elephant, one might naturally expect to meet with a brain comparatively small;—small, because the propensities which form the animal's incitement to action, and which commonly compose the greater part of the encephalon, are here weak and less important. At the same time, the intellectual part, and the sentiments immediately connected with it, must be comparatively well developed. The fine skull of an Asiatic elephant, now in the Philadelphia Museum, corresponds, as far as its inner surface can be examined, entirely with this description. The thickness and unevenness of the bones are so great as to render it impossible to judge, from without, of the internal form; but by introducing the arm through the occipital foramen, it is found that very little indeed of the cavity lies behind the ears, that the width and elevation of the cavity are moderate; but that, on the contrary, the anterior extent in the direction of the organs of intellect, is very considerable.

Many of the striking exceptions in the table might, perhaps, be explained, were we as well acquainted with the character

of the animals as we happen to be with that of the elephant; but it is still very evident that we have not as yet arrived at the simple and general principle. This has led the investigating mind of Sœmmering to the consideration of the proportion between the brain and the nerves which issue from it. In this pursuit, he finds a principle which appears at present to afford *no exception.* After examining the brains and crania of a great number of animals, this distinguished anatomist informs us, that " in no animal had he ever found an encephalon, whose proportion to the nerves connected with it, made any approach to that of the human subject."*

To understand this it is necessary to premise, that the weight or bulk of the nervous system is totally incapable of being ascertained with sufficient accuracy to give a table of proportions, similar to that of which I have above read you an extract. The nervous branches can neither be dissected entirely out, nor can any certain proportionate part of them be ascertained. The only means which it is in the power of

* Sœmm. Icon. bas. encephali, p. 6. As no one that I have met with has copied this author's list of the animals which he examined, I shall furnish the names.

Simia cynamolgus, aigula and lugubris of Erxleben, papio mormon, cercopithecus jacchus, lemur mungoz, three-toed sloth, common hedge-hog, hystrix cristata, murine bat, European and flying squirrel, marmota citellus and cricetus, mouse, rat, European mole, rabbit, hare, cavia porcellus and aguti, mustela martis and putorius, melius taxus and lotor, bear, dog, wolf, fox, cat, tiger, horse, ass, mule, dromedary, goat, capra ovis ammonis, chamois, bull, cervus dama, elaphus and capreolus, hog, elephant, beaver, otter and seal.

Besides the brains of the above, he examined the crania of the simia satyrus, longimanus and cynocephalus, papio mandril, cercopithecus paniscus, lemur macaco, myrmecophagus 3-dactylus, manis macroura, tatuus 9-cinctus, various bats, glis esculentus, sorex aranea, didelphis dorsigera and marsupialis, jerboa, civet, ursus maritimus, hyæna, lion, tiger, leopard, lynx, camel, ibex, antilope recticornis, bezoartica, oryx and strepsicerus, bison, buffalo, musk ox, camelopard, cervus alce and tarandus, moschus moschiferus and pygmæus, sus æthiopicus, tajassu and babirussa, tapir suillus, one and two horned rhinoceros, hippopotamus, phoca ursina, trichecus rosmarus and manati, narwhal, balæna mysticetus and physalus, physeter macrocephalus, delphinus phocæna, delphis and orca, and the fossil species of the elk, bear, and rhinoceros; together with a great number of birds, amphibia and fishes.

the anatomist to use, is to measure the size of the nervous branches at their origin; and thus gain the proportionate development of the brain to that of the means provided by nature for connecting its operations with those of the rest of the frame. In this manner, when the brain of an animal was not itself to be obtained, the skull, as in the instances last enumerated, afforded a very good substitute, by means of the foramina or apertures through which the nerves penetrate the bone. As most of these are exactly filled by the nerves and their appurtenances, they are proportionate to them in size.

Besides the species which Sœmmering actually examined, the observation can be extended to birds in general; in consequence of the very great size and powers of the organs of sense in those animals. The large eyes and optic nerves of birds of prey and many others, the great nerves extended on the nostrils and beak in the anseres, and the extreme perfection of the organ of hearing in singing birds, all render probable the further extension of this principle in that tribe of animals. I shall exhibit to you a few examples of this observation in the specimens which are before you. This is a remark particularly necessary, in consequence of the great size of the cranium in some of these animals, sometimes exceeding, as in the instance of the canary bird, the proportions of the human brain. Is it not obvious that the impressions transmitted by these powerful organs of sense must require a proportionate development of the brain to receive, judge, and direct them? The nervous system of birds, then, corresponds to that of *larger species*, &c. among the tribes of mammalia; and, in this respect, finely agrees with the greater warmth and aeration of their blood, the greater vivacity of their temperaments, and the greater relative size and strength of their muscular systems of voluntary motion.

The proportion of the brain to the nerves of sense which originate within the cranium, is generally shown by that of the cranium to the face; the latter being composed of the organs which these nerves supply. Hence arises the celebrated principle of the facial angle; which, as has been

shown by Camper, not only extends to men and the brute creation, but has been acted on by the ancient artists, and applied to their representations of their heroes and their gods. When we see a Phidias or Praxiteles expressing dignity and elevation of soul by a forehead, lofty and projecting in comparison with the rest of the face, we cannot avoid being struck with the confirmation which it affords to the ideas both of Camper and of Sœmmering.

In this consideration, however, we omit one of at least equal importance, that of the proportionate size of the brain and spinal marrow. With the very extraordinary and solitary exception of the dolphin, I believe there is not a case known in which the principle adopted by Sœmmering does not hold good: from the complicate brain and smaller nervous system of man to the insect or molluscum, the knots on whose nerves are so nearly equal in size that we can scarcely fix upon one larger than another, to which to affix the title of brain.* The proportionate size of the brain to that great nervous trunk which forms its medium of communication with all the lower parts of the body, is in every instance such as tolerably to correspond with what we know of the animal's propensities and mental powers.

Of the other peculiarities accompanying the strange and

* The following is what Cuvier has furnished us in his "Leçons d'Anatomie Comparee," to illustrate this subject.

The medulla behind the pons varolii is to the brain, in width, in

Man, as	1:7	Ox	5:13
Bonnet chinois	1:4	Calf	2:5
Macaque a queue court	1:5	Horse	8:21
Dog	6:11 or 3:8	Dolphin	1:13
Cat	8:22	Falcon	13:34
Rabbit	3:8 or 1:3	Chouette (species of owl)	14:35
Hog	5:7	Duck	10:27
Ram	5:7	Turkey	12:33
Stag	2:5	Sparrow	7:18
Roe-buck	1:3		

The reader will observe, 1. that the proportions of only one cetaceous animal, namely, the dolphin, are given; and 2. that the large brain of the sparrow, the only one of the remarkable anomalous instances in the other table, which is mentioned here, is accompanied with a proportionately large spinal marrow. The whole nervous system of this small bird is proportionately far larger than that afforded to the larger species.

striking anomaly attributed to the dolphin, and of their influence upon the question, I cannot precisely speak, till I have the opportunity of dissecting one: from Cuvier's description, however, they seem to be very great. "Le cerveau du dauphin est d'un forme tres extraordinaire; ses hemispheres sont fort epais; il recouvre le cervelet pardessus; il est arrondi de toutes parts, et presque du double plus large que long." In the meanwhile, however, exceptions of this kind are far from being conclusive against a rule. They have not the force which a negative possesses in general reasoning; as the anomalies of nature are so endless that exceptions only prove our ignorance of the particular case, or of some new and hitherto unknown class of cases to which it belongs. In so unusual and extraordinary a deviation from the analogy of land animals as that of the dolphin, a member of a class so distinct in various respects, and which correspond with it in many of its peculiarities, it is not matter of surprise that there should be a disproportion of this kind, in its widely dilated and distorted brain.*

It may be useful, before leaving this subject, to take a view of the conclusions to which those who are fondest of the opinions we are endeavouring to disprove, would lead us. Arguments similar to those which we have been discussing are used by many of those who attempt to prove that the mind has no connexion at all with the brain, that the latter is an organ whose use is entirely unknown, and that the mind is totally independent of matter. We must discharge Dr. W. from the accusation of holding such dogmas as these. To others, however, we must merely in passing observe, that what is called the common sense of a large portion of mankind considers these opinions as absurd; that the language and familiar phrases of many nations, testify at least a general conviction that the mind and the brain are directly and intimately united, and that

* The cerebral cavity in a skull of a porpoise, in my possession, measures in length, 4.6 inches; in breadth. 6.2! The occipital foramen measures 1.38; giving a ratio of 2:9 or 1:4 49. The medulla must, of course, have been still smaller The cavity is evidently brought into this short and wide form, to make room for the *blow holes*, a characteristic of this tribe of the mammalia.

in short, there is no danger that these opinions will ever gain many proselytes. It is not now necessary to go over the numerous proofs which science and every day experience afford to the connexion of the mind with the brain; and we may safely, in assuming the general belief in the fact, warn those who hold it, narrowly to inspect a series of observations the ultimate drift of which is to gainsay so notorious a truth.

We have here arrived at a principle of great importance in judging of almost any of the arguments relative to phrenology, and yet one much neglected by some of those who oppose it. It is that of the *relative* size of parts. It matters little, in forming a peculiar character, that we suppose a man or an animal endowed with a particular propensity in a great degree, unless at the same time we make it predominate over others. Where this is not the case, it only infers the cotemporaneous existence of greater force of passion, and not a character marked by any peculiar dominant propensity or habit of acting. We are told by Dr. Warren, in his late publication, that he found the skulls of animals of the most peaceable and mild temperament wider at the situation of the alleged organs of combativeness than those of the lion and of large dogs.

"The cavity of the cranium in the lion and large dogs is oblong in a direction from before backward; the skull is narrow at this part, and the appearance spoken of does not exist in the bones. In the skulls of two lions in my possession, and in various large dogs, the cranium is more narrow at this part than in the skulls of various monkeys, and is not materially broader than in the sheep. In birds, the cranium of an owl is broader than that of an eagle." *Comparat. View*. It is obvious that no allowance is here made for the *proportion* between the supposed faculty and those which counteract it. Animals of different sizes, both of brain and body, are lumped together in comparing merely the absolute width. As the cranium of the sheep is also known to be of a very irregular shape and thickness, and fortified with a thick, projecting ridge at the widest parts, the pro-

fessor should have compared the cavities; which he does not inform us of his having done.

We have also no evidence that the owl, well known to be a ferocious, destructive and carnivorous bird, has less courage than the eagle. This is another confusion of superior size with superior ferocity. We shall observe further along that birds afford but very ambiguous examples.

It might excite surprise that Dr. W. would be content with so partial and imperfect an examination of the point; but it is to be presumed that the very general nature of his work did not allow him to devote a sufficient share of attention to a subject which formed only a small part of it. I have not been able to obtain a sight of the skull of a lion; but there are in your collection very fine specimens of those of the royal tiger and the tiger-cat of Bengal; and I have also examined those of a number of different carnivorous species; among which are several of the feline tribe, dogs, foxes, bears, the jackal and the wolf; the greater part of which are in the Philadelphia Museum. In all these, without a single exception, there is a striking peculiarity of form, consisting in the very decided and obvious prominence of that portion of the skull which has been marked as the organ of destructiveness. To this may be added, with the exception of the cat tribe, that of the propensity to secrecy, unless, indeed, as those organs whose situation is not remarkable are hard to identify on the heads of brutes, we suppose this faculty deficient, and the organ of cautiousness, which is here difficult to distinguish from it, to usurp its place. In the small and timid variety which is kept as a lap-dog, the two specimens which I have examined, have the spaces corresponding to destructiveness, secretiveness and cautiousness large, but increasing to a remarkable size in the place of the latter; all which corresponds to the animal's well known character.* In the skull of a sheep,

* Specimens of all the species and remarkable varieties of animals mentioned above, were here exhibited, and the peculiarities shown, to the expressed conviction of many gentlemen, including one who had entertained a different opinion. It is believed that acquiescence as far as the specimens were examined, was unanimous. In fact no one who has seen the heads has doubted a moment.

which I formerly had in my possession, but which I have been disappointed in replacing for your inspection to night, it was hard to find a corresponding structure. The size of the whole cerebral cavity was greater; but it did not afford these predominant indications. On the other hand, it seemed to be proportioned to the general great size of the head, according to the remark of Sœmmering, and particularly to the enormous development of the olfactory nerves; and it also serves to show, in conformity with a remark which we have made before, that a greater degree of mental power and of general force of impulse may be intrusted to innocent animals than to beasts of prey. Were the latter to be more artful and intelligent than they are, were they to be at once gregarious, cunning, and cautious, they would soon exterminate the other species from the earth. We may judge of the havoc which feline animals would produce, under these conditions, from that executed by wolves, an animal individually much less active and ferocious. How would our back settlements thrive, were they infested by troops of panthers?

The attribute of combativeness, or a disposition to fight without advantage, which is very little indicated on any of the heads of animals which I have seen, is much more sparingly distributed to animals than is frequently supposed. Buffon, I think, has remarked that he doubts whether any animal but the dog will voluntarily and without extraordinary excitement attack one its equal in size and prowess. To this we may add that the dog always expects to be assisted. It at least admits of a question whether man is not the most courageous animal on the earth. He is the only one who rushes coolly on certain death; he is the only one, since the story of the scorpion was exploded, who voluntarily destroys himself, when under the pressure of calamity; and that too, in defiance of the terrors of futurity; and finally he is the only one who is fully sensible of the certainty of approaching dissolution, and of the full amount of his temporal distresses, in those cases where he, in a manner emphatically called manly, struggles to the last. The

tremendous combats of the lion and tiger depend more for their terrors upon bodily strength than on that courage which is never tried by the presence of a superior. Their object, unless when defending their young or when individually driven to extremity, is the destruction of creatures inferior to themselves in strength, activity and weapons.

Dr. Warren quotes from Spurzheim the words, "courageous animals have the head between and behind the ears very large," and then proceeds to compare the width of those of the sheep and the lion. It is necessary to correct the mistaken impression to which this has given rise—that the remark is made generally or in general terms. The phrase occurs thus:

"Even individuals of the same kind differ entirely with respect to this faculty. One dog incessantly looks for an opportunity of fighting, another always flies away. *The courageous animals have the head between and behind the ears very large.*" *Physiog. System. p.* 375. It is obvious that Dr. Spurzheim is here speaking of the comparative width behind the ears, of different animals of the *same species*, and not of animals different in their kinds. Although, however, it was not applied by that writer in so general a comparison, the remark will be found to hold good to a very considerable extent, that ferocious animals are, in proportion to the size of their heads, wide between the ears. This is seen in a striking manner in the front view of the cat, the fox and several other animals; giving the face nearly the form of an equilateral triangle, of which one angle is at each ear and the other at the tip of the nose. As this physiognomical rule, however, is not anatomical in its nature, it is liable to exceptions and objections. Dr. Warren observes, and I believe justly, that the great apparent width of the head of the lion at this point arises from the enormous thickness of the temporal muscles; and I am fully convinced that the appearance marked in Spurzheim's tenth plate may be, and very probably is, produced by the same cause. The remark is exactly true of all the rapacious animals whose crania I have examined. The distance of the ears may depend on the interposition of either bone alone or

bone and muscle combined; and hence it is reasonable to expect deviations from such a rule, as it does not certainly indicate any circumstance respecting the brain. At the same time, as a physiognomical remark, it is generally correct. The appearance we above described arises from three conspiring causes; 1st, the width of the part of the scull considered as the organ of destructiveness in these animals; 2d, the great size of the temporal muscles common to them all, to enable them to use with effect that formidable weapon, their teeth; and 3d, the shortness of the face, for a mechanical advantage having the same object, and from the absence of the disproportionate organ of smell possessed by herbivorous animals.

Of the organ of tune I cannot speak, having no knowledge of it from men or animals; and I have always considered it as one of the less ascertained organs. The brain of birds, in particular, is of so dissimilar a structure with the human, having no convolutions, that it appears to me no comparison can hold. As the whole of the convoluted surface is here wanting, some other structure of course performs those of its functions which are necessary; and we cannot draw strict analogical inferences from any of its appearances. This, therefore, I am willing to dismiss.

The organ of amativeness is but lightly touched upon by Dr. Warren. He says nothing of the effect of castration, or of the difference between the male and female of polygamous animals; but contents himself with copying nearly all of Cuvier's table of cerebella, and intimating that it does not coincide with phrenological views.* He observes, that "in monkeys generally, it is much less developed than in

* The following is Cuvier's table of cerebella. The cerebellum is to the cerebrum in weight, in

	Man as - - -	1:9	The Mole - - -	1:4½
Monkeys and Baboons.	The Saïmiri - -	14	Beaver - - -	3
	Saï - - -	6	Rat - - -	3¼
	Magot - -	7	Mouse - - -	2
	Papion (common baboon) - -	7	Hare - - -	6
			Boar - - -	7
	Mone - -	8	Ox - - -	9
	Dog - -	8	Sheep - - -	5
	Cat - -	6	Horse - - -	7

man; and in the baboon, the most extraordinary of all animals for the propensity, it is in no way remarkable;" and again, that "precisely the same proportion exists in the baboon and in the horse, animals differing widely in the degree of this propensity." This, and the fact, that "the proportion of the cerebellum is in many animals, greater than in the monkeys," constitute all the arguments which professor Warren has adduced on the subject.

It is eminently difficult to form correct conclusions on this point, as further knowledge is wanting, both of the proportions of the cerebellum in animals whose sexual appetency is well known, and of the violence of propensity in those whose proportions are here given. Cuvier's table, which is evidently not made for the purpose of elucidating this question, is extremely barren and incomplete. Of sixteen animals enumerated, five are baboons and monkeys, of which we shall speak anon; and four belonging to the order rodentia, whose cerebral system is very peculiar; differing widely from the structure of most of the mammalia, and approaching that of birds. The thick, expanded folds, which constitute the circumvolutions, are almost entirely deficient in these animals, and as their brain is of greater size than appears to be indicated by their intelligence, we are quite ignorant how far this increase of size may be a mere compensation for a different and less compounded structure. They are brains on a different principle from the others, and we cannot apply the same rule of proportion. We may even say, that the cerebellum being deprived of the greater complexity of structure, which it possesses in the higher animals, is placed much more in the above condition than the rest of the brain; and, therefore, may be expected to present a larger size.

To return to Dr. Warren; the table certainly does not show, that "in the monkeys generally, it is much less developed than in man;" this being there stated only of the Saïmiri, a species remarked for its gentleness and quiet disposition. I am quite ignorant, therefore, whence he got the fact.

As to the common baboon, although well known to be an

animal of extraordinary salacity, yet Audebert* does not describe him as greatly exceeding the animals of the same subdivision, such as the magot; and until we can get particular information of the habits of the saï and the mone, we cannot judge further than as to general probability with regard to them.†

Professor Warren compares the baboon and the horse, as "animals differing widely in the degree of this propensity." Of what example of the latter the professor speaks, I cannot judge; for certainly the salacity of a stallion is proverbially no weak propensity; though the mind, without particular recollection, may be influenced by the daily habit of witnessing the character of the mutilated animals we commonly ride. The remaining species in Cuvier's table, which exceed, in this respect, the proportion of the baboon, are the saï, of whose salacity I know nothing; the cat, the violence of whose amatory passions is well known; the sheep, of the male of which, the same may be said; the mole, and the four rodentia, of which we spoke before. The difference of structure and consequent defect of the analogy in these animals, I have already noticed: of their manners, I presume it will be very difficult for any one to judge; for who possesses personal knowledge of the sexual appetites of the beaver, the rat, or the mouse?

It is, I think, now pretty obvious that we do not possess materials by any means copious, for the illustration, from the comparison of the cerebella and amatory propensities of the mammalia, of the final question of their connexion. The argument drawn from the universality of the presence of such a body in animals that copulate, together with all those arising from observations in cases of disease and injury, during excitement of the propensity, and in remarkable individuals, yet remain. Feeling that degree of uncertainty,

* Hist. des Singes, Art. Papion.

† The apparent violence of passion in all these creatures, is also greatly increased by confinement from their accustomed exercise and amusements, and by the means they possess of displaying it, beyond all other brutes, and in the absence of the females.

which I conceive arises from the state of the evidence, I must still express the opinion, not merely that the subject is a legitimate and promising field of inquiry, but that it is from observations on the peculiarities of animals, that light is most likely to arise on it. The relative size of the part, its different degree of complexity in structure, the difference of male and female in polygamous animals, and above all, the actual differences of dimensions and weight in castrated animals, as well as the exact habits of a number of species, must all be ascertained before we can approach any thing like certainty on this curious question. The weight of the part in different creatures can only be with exactness compared, when there is something of similarity in the structure; and this, if we include man, only extends to quadrupeds, exclusive of the glires, and probably of some others; and, perhaps, to some of the cetacea. How little is known of all this, is evident from Cuvier's table, which includes nearly all that we possess respecting these facts, and which is totally unaccompanied with descriptions of the characters of the species which it enumerates. Before we quit this subject, it may be well to hear the opinion of the great man whose labours have furnished the ample, and, in fact, the only materials employed by our adversaries in attacking these parts of phrenology. Authority is not my idol, nor is it a test of truth; but those who are disposed to value it so highly, and to draw so many and extensive inferences from the collections of Cuvier, should hear what that distinguished improver of physiology has said, at a time when, as is evident from the context of his work, he had not taken up the inquiries of phrenology.

" Il paroît aussi que l'on entrevoit certains rapports entre les facultés des animaux et les proportions de leurs parties communes. Ainsi la perfection de leur intelligence paroît d'autant plus grande, que l'appendice du corps cannelé qui forme la voûte des hémisphères est plus volumineux. L'homme a cette partie plus épaisse, plus étendue et plus reployée que les autres espèces. A mesure qu'on s'éloigne de l'homme, elle devient plus mince et plus lisse; a mesure

qu'on s'éloigne de l'homme, les parties du cerveau se recouvrent moins les unes les autres; elles se développent et semblent s'étaler d'avantage en longueur.

"Il paroît même que certaines parties prennent dans toutes les classes un développement relatif à certaines qualités des animaux.—On peut espérer, en suivant ces recherches, d'acquérir quelques notions sur les usages particuliers à chacune des parties du cerveau ou de l'encéphale."—*Leçons d'Anat. Comp. v.* 2, *p.* 175.

I hope enough has now been said to convince you that the reviewer is not borne out in the opinion which he expresses, that, by so slight an inquiry as the one which we have been examining, "those who have made triumphant *assertions*, that they were supported in the doctrine of craniology by comparative anatomy, are now entirely deprived of its aid." The form and arrangement of the brute cranium are so different from those of the human, that it is, in most instances, difficult to determine the precise points which correspond to the organs delineated upon the human head; but a doctrine which has so many relations to comparative anatomy in the internal structure of the brain, as well as in its mere form, is not "deprived of its aid," because Dr. Warren could not find combativeness, tune, and amativeness to correspond, and because the reviewer did not repeat his inquiries.

I feel compelled, gentlemen, again to apologise to you, and particularly to those of you who are not members of this society, for allowing this lecture to assume so much the form of a review of a scientific work. It arises from the peculiar situation of us and of our science at the present juncture. Our pursuits are looked upon by many with so unfavourable an eye as almost to amount to a persecution. Some object to phrenology as a thing too ridiculous to be a serious object of study; others, as a great evil, tending to the subversion of religion and morality. While we are not indifferent to the opinions of our fellow citizens, we shall, and should certainly endeavour to keep our minds open to evidence as respects either side of any question relating to

phrenology. It is therefore necessary, for preserving the due balance of the minds of inquirers, occasionally to reply to attacks involving the general question of the propriety of the study. To observations which, like those of professor W., refer to particular applications of the doctrine to nature, with a view of testing the correctness of the locality assigned to individual faculties, or other of the different parts of which the doctrine consists, no reply is, in general, requisite; as all that is needed, or even, in fact, desirable, is the further extension of such inquiries, they being the only true method of ascertaining the amount of truth and error in this system. But when, as in the present instance, sweeping conclusions, as to the entire absence of foundation for the science, even in a small part of its relations, are drawn from inquiries legitimate enough in themselves, in objecting to those latter we are obliged to investigate the origin in which they are founded.

While, however, in the act of discussing a scientific question, thus in a great measure forced on us, I usurp the privilege of a reviewer, and attempt to show the insufficiency of Dr. Warren's arguments against comparative phrenology, I by no means relinquish the privilege of expressing an entire accordance with the general opinion of the work pronounced in the article in the Philadelphia Journal. It is a work possessing excellencies of various kinds, and calculated not only to do honour to the author, but to add dignity to the profession, of which he is a member. The medical literature of this country is too frequently deficient in the elegance and correctness so conspicuous in the book before us. At the same time it is a very general work, which has evidently been written, in a great measure, from the stores of physical knowledge which enrich the memory of its eminent author, and without any very extended research having been made for the special purpose of composing it. It is wrong, therefore, to elevate it above the labours of the rest of the physiologists, and particularly so as respects the nature of the discussion. These inquirers into truth are represented

in the commencement of the review, as being "busy in disputing on the *location* of the human mind in a certain part of the body, and *appealing* to *assertions* made by authors in defence of their opinions;" while "our distinguished countryman has been engaged in the examination of facts, and in the interrogation of nature herself." What the citation of tables from Cuvier is, other than an "appeal to an author," I am unable to say; for I cannot suppose that the reviewer imagined Dr. Warren to have made these tables from his own observations; but a modest book, only pretending to be a "*comparative view*" of what is known on the subject, containing no experiments, and few or no original observations, cannot claim such high ground in comparison with others. It is also well known that the physiologists of the present day deserve, perhaps, less than at any former time, the above reproach of not appealing to observation. Several of the greatest medical names of the present era, besides a host of minor inquirers, have gained their celebrity, and are still increasing it, by being diligently employed in interrogating nature.

The review is evidently written with honourable feelings, and, generally, in a correct tone of scientific remark; liable only to objections which have probably arisen from the haste with which it has been penned. I regret, on the reviewer's account, as well as that of the original work and its author, having been compelled to use the language of censure which was rendered indispensable by the controversial nature of the subject, and by the powerful influence which discouraging opinions, emanating from a source of authority and weight, unquestionably have upon the inquirers into phrenology.

In an article upon Lawrence's lecture, on physiology, published since the above was read, in the North American Review, there occur the following remarks, connected with this subject.

"We hinted above our opinion of the doubtful authority of Gall and Spurzheim; in proof of this, we shall borrow the notice of two mistakes respecting important organs, from Dr. Warren's late work on the Sensorial and Nervous Systems. These authors say, that the organ of courage resides in the space between and behind the ears, and that 'courageous animals have the head between and behind the ears large.' This is probably founded on a hasty examination of powerful animals, such as the lion, whose head in this part is really broad, on account of the large muscles which cover it. When these are stript off, the skull is seen to be narrower than that of various monkeys. And the organ of amativeness is in no way remarkable in the baboon, the most extraordinary of all animals for this propensity. Lastly, when Dr. Spurzheim, at his lectures in Edinburgh, was several times publicly defied, by the late learned Dr. Gordon, to demonstrate some pretended discoveries respecting the cause and origin of the nerves, he was unable to return any satisfactory reply."

The reader will observe, that all the statements in this extract, with the exception of the last, have been already replied to in the lecture. On perusing the concluding sentence, one can hardly imagine that the reviewer has seen Dr. Spurzheim's able and laborious reply,* of about one hundred closely printed pages, which was published soon after.

In this work Dr. S. rebuts, separately, every article in which Dr. Gordon attacks him, and the resulting impression, in my mind, from having very carefully read both the *printed* attack† and the reply, and made several dissections with a view to verify them, is, that, excepting the structure of the zig-zag, or ashy-red coloured body in the crura cerebelli, and the assertion that the optic nerves could be traced to

*Examination of the objections made in Britain against the doctrine of Gall and Spurzheim. By J. G. Spurzheim, M. D. Edinb. 1817.

† Edinb. 1817.

the testes in man, as they can in brutes, the violent and intemperate assaults of the latter writer were completely defeated. The pedantry with which Dr. G. refers the different shades of the cerebral matter to Symes's nomenclature of colours, the extreme importance which he attaches to trifling distinctions, and the unnecessary acrimony of his reflections, all furnish evidence of a want of the judgment requisite for such a discussion. Many of the alleged anatomical discoveries of Dr. S. are founded upon comparative anatomy, and the structure can frequently be displayed with facility in animals, while, in the human subject, it can only be done under favourable circumstances. A great number of the conclusions to which he comes have been demonstrated in Philadelphia; particularly by the late Dr. J. O'B. Lawrance, who was as cautious in drawing inferences from observed facts as any inductive reasoner perhaps in the world. The fibrous structure of the whole brain, the capacity of the convolutions to be unfolded, the continuation of the divergent fibres of the pyramids into the striæ of the corpora striata, &c. are now familiar exhibitions in our schools. The reviewer is possibly not aware that a committee of the French national institute, in a very long report to that body, furnish a very considerable list of discoveries in the anatomy of the brain, which they freely attribute to Dr. Spurzheim.

Various pieces have been shown me, in periodical journals, newspapers, &c. written against phrenology; and all attacking the fundamental principles of its reasoning. In fact this notice of professor Warren is a remarkable exception from the general custom. No such painful and tedious comparisons with fact are thought at all necessary. Nothing but root and branch work will please these critics. Indeed, it is often calculated to excite a smile to see how easy a victory is anticipated; how little inquiry or reflection is thought sufficient radically to overthrow a doctrine which certainly cannot be denied to have employed some study. It would be prolonging this article quite too much for the

patience of our readers, to undertake to answer all these. It is sufficient to remark, that the objections hitherto made, in the publications to which I allude, being generally founded on ignorance of what they attack, vanish with extraordinary facility the moment they are brought to the light and fully explained. It is now absolutely necessary, in order successfully to oppose phrenology, to understand something of what is to be overthrown; a condition which requires some attention to the subject, an exertion which with many is sufficient to stamp the student with the opprobrium which they labour to attach to the science.

CASES.

Art. V. *Case of Rupture of the Axillary Artery, in a successful attempt to reduce an old luxation at the shoulder joint* By W. Gibson, M. D. Professor of Surgery in the University of Pennsylvania.

James Scofield, fifty years of age, of intemperate habits, and foreman to the Penns-Grove Cotton Factory, on Chester creek, applied to me on the 10th of May last, on account of a dislocation of the left arm, at the shoulder joint, produced two months before, by the weight of a heavy chest, which fell upon him, from a cart, while he was driving it along the road. A physician was immediately sent for, who stated that the arm was fractured just above the elbow, and must be secured by splints and bandages. These were accordingly applied, and continued about two weeks, when the bone was declared so far united as to render the dressings unnecessary. No notice, according to the patient's account, was taken of the *shoulder*, although, from the first, the swelling had been considerable, and the pain very severe. A short time afterwards the patient consulted Dr. Dutton, of Village Green, Delaware county, who, discovering that the os humeri had been luxated at the shoulder, and still remained displaced, determined to make an effort to restore it to its natural situation. With this view the patient's body was securely bound and rendered immoveable; three pints of blood were drawn from the right arm, whilst a strong sheet was twisted around the injured arm, above the elbow, and its ends given to five strong men, who were directed to keep up a constant and steady extension, which was continued for some time, and frequently repeated, but without any benefit. The patient suffered, as he remarked, a good deal, from this attempt to restore the bone to its

place, and was debilitated by the loss of blood; still he was willing to undergo any torture, provided there was the slightest probability of his arm being again rendered useful. For this purpose he came to Philadelphia, and consulted Dr. Humphreys, by whom he was referred to me. It was evident, upon examination, that the head of the os humeri had been separated for a considerable time, from the glenoid cavity; for I found it so firmly lodged in the axilla, that the arm would scarcely admit of any motion, and the slightest movement occasioned pain. After explaining to the patient the uncertainty of any benefit resulting from a further attempt to reduce the bone, and pointing out to him the suffering that must necessarily follow the efforts to restore it, I determined to make the trial, and for this purpose requested him to meet me on Monday the 12th of May, at the Alms House. Having arranged the necessary apparatus, I desired Mr. Gregg, one of the house pupils, to bleed the patient in the right arm. While the blood was flowing, a buckskin band, with an iron plate and ring secured to it, was fastened around the wrist. A large roller was then fixed in the arm-pit, and over this a sheet, folded diagonally, the ends of which were carried before and behind the chest, towards the opposite shoulder, and fastened to a hook. This sheet served for the counter extending band. Pullies were next attached to the ring at the wrist, and every thing being prepared, I commenced the operation, (in presence of Drs. Humphreys, Horner, Jackson, the resident physicians, and students of the house, and several other spectators), by setting the pullies in motion, and keeping up, for several minutes, a continued but steady extension and counter extension. This fatigued the muscles of the arm considerably, and the patient was sensibly affected by the loss of nearly two pounds of blood, but did not faint. I then relaxed the pullies, and taking hold of the arm, near the elbow, used it as a lever, and communicated a rotatory motion, in hopes of breaking up the adhesions and adventitious ligaments, connecting the head of the bone to its new socket. Additional attempts were made with the pullies, apparently with-

out the slightest effect. Dr. Horner now proposed to change the direction of the force of the counter extending band, by fastening a hook in the floor, seating the patient on a chair, and passing the middle of a strap over the point of the acromion process, in order to secure the scapula. This was, also, tried, but with no better success. I next disengaged the extending and counter extending bands, and laying the patient out upon the table, placed one of my heels in the axilla, while I produced extension, by pulling at the patient's wrist. The same was done by house pupil Strudwick. Finding these efforts unavailing, another attempt was made by means of sheets, fastened above the elbow and under the armpit. Five or six assistants took hold of the ends of each, and pulling steadily for some time, the head of the bone was perceived, gradually, to yield. It quickly returned, however, nearly to its former position, as soon as the efforts were discontinued. By this time the patient was greatly exhausted, and the muscles very much relaxed, when Dr. Horner requested him to lay on the floor, and at the same time stretched himself down opposite to him, and taking hold of the wrist, made a continued but forcible extension, while counter extension was effected by his heel in the axilla. During these efforts the head of the bone gradually approached the glenoid cavity, and at last entered it. The slightest movement, however, was sufficient to throw it out again, which led me to suppose that a portion of the capsule might be interposed between it and the socket, and would require further laceration before the reduction could be entirely accomplished. But the patient was too much overcome to make any further attempt at that moment, and was therefore put to bed. On visiting him half an hour afterwards, with Dr. Humphreys, I found the head of the bone resting on the lower edge of the glenoid cavity, and a hollow under the acromion. I took hold of the arm, and made two or three slight rotatory motions, when it slipped suddenly into its place, and was completely reduced. There was a general swelling about the deltoid and pectoral muscles, which was noticed both by Dr. Humphreys and myself, but

supposing it to be an approach to inflammation, a consequence to be expected after the efforts made to restore the head of the bone, nothing was apprehended from it. The swelling increased, however, very slowly, for several hours, and although remarked by the house pupils and attendants, did not excite any alarm, inasmuch as the patient complained of little pain, and conversed cheerfully with some of his friends during the greater part of the afternoon. About six o'clock in the evening Dr. Brinton, one of the house pupils, visited him, and hearing that he had a short time before turned over in bed, in order to sleep, and struck with the unusually pallid appearance of his face, was induced to suspect that some unfavourable change had taken place. These suspicions were confirmed, for upon examination the pulse was found scarcely perceptible, and the whole system so much sunk, as to render recovery impossible. Leaving Dr. Hopkinson in charge of the patient, Dr. Brinton immediately repaired to my house, and informed me of his condition. Before I could reach him, however, he expired. The appearance of the shoulder and adjacent parts soon explained, it seemed to me, the nature of the case; for the pectoral muscle was considerably elevated, and the skin, for some distance about the chest and shoulder, discoloured and ecchymosed, showing, in all probability, that some large artery or vein had been torn across, during the efforts to reduce the luxation. To determine this point with accuracy, I obtained the consent of the patient's friends to examine the body, and at ten o'clock next morning the dissection was made by Drs. Horner and Lawrance, in presence of Drs. Humphreys, Jackson, the house pupils, several students, and myself.

Dissection.

Three incisions were made—one from the acromion process, along the course of the clavicle, as far as the sternum—another perpendicular to the sternum, and about ten inches long—a third nearly at right angles with the lower extremi-

ty of the perpendicular one, and running across the chest towards the arm-pit. The teguments and pectoral muscles being elevated along the edge of the sternum, and thrown backwards towards the shoulder, a considerable quantity of coagulated blood was found, filling the cellular membrane, and laying in masses between the interstices of the muscles. In order to ascertain the condition of the large vessels beneath the clavicle, this bone was separated at its juncture with the sternum, and raised. The course of the subclavian artery and vein was then distinctly seen. A small opening was made in the vein, into which a bougie was introduced for several inches, towards the axilla, as a guide during the dissection; but the vessel was found perfectly sound throughout. Under the vein, as it passes near the glenoid cavity, a large mass of coagulated blood was observed, and upon clearing this away, the axillary artery was seen protruding, with its mouth open, having been torn directly across and separated from its connexions. Upon further examination, it was discovered that the head of the bone, at the time of the luxation, had been carried downwards into the axilla, about an inch and a half below the glenoid cavity, where it formed a white ligamentous cup-like socket, in the subscapulary muscle, and pressing upon the axillary artery, produced such a degree of inflammation as gave rise to a copious effusion of coagulable lymph, which united the artery completely for some distance, to the capsule of the joint, where it surrounded the neck of the bone. The lower part of the capsule was torn and separated from the neck of the humerus; the upper part remained entire, and was very much thickened. The head of the bone filled completely the old socket or glenoid cavity. Beneath the deltoid muscle there was a large hollow filled with blood, and the whole arm, as far as the elbow, had been extensively injected with the same fluid. The os humeri was carefully dissected from the condyles to its head, and the periosteum entirely scraped off, without showing the slightest vestige of a fracture. The long tendon of the

biceps was found considerably elongated, but not ruptured.*

Remarks.

The foregoing case must be considered in every point of view, extremely interesting; it was mistaken, it appears, by the physician who first saw it, for a fracture near the elbow, and treated accordingly; a few weeks afterwards the true nature of the disease was discovered by another practitioner, and an attempt very properly made, but without effect, to restore the head of the bone to its natural situation. The patient finding his arm useless, and unable to follow his occupation, determined, notwithstanding his previous suffering from one operation, to submit to another. The trial was made, under every disadvantage, the head of the bone restored to its socket, the axillary artery torn across, owing to an accidental adhesion between it and the capsule of the joint, which could not be foreseen, and the patient died. Persons acquainted with the difficulties often encountered, even in the most simple cases of luxation, will readily understand, without comment, the peculiar nature and the inevitable result of the case I have detailed. For those who possess little practical information on the subject, and who may, perhaps, be led to condemn the efforts to relieve the unfortunate patient, as rash and unwarrantable, the following observations are chiefly intended.

The head of the humerus may be forced from the glenoid cavity of the scapula, and lodged in different situations. In nine out of ten cases, however, it rests in the hollow of the arm-pit, having previously ruptured the inferior portion of the capsular ligament. The tumour, formed by the head of the bone, in the axilla, and the unnatural hollow under the acromion process, are signs so decisive of the nature of the accident as not to be overlooked, except by the most careless or ignorant practitioners. To restore the bone to its

* The diseased part being removed, was carefully prepared by Dr. Lawrance, and deposited in the surgical cabinet at the University, where it may at any time be inspected.

original position, the surgeon makes *extension* and *counter-extension*, either by the hands of strong assistants, some of whom take hold of the dislocated arm and pull steadily, but forcibly, while others resist, by securing the body or shoulder, or by towels, or sheets, straps, or pullies, as the case may require. If the force be well directed, and continued sufficiently long to fatigue the muscles, and thereby overcome their resistance, the head of the bone generally slips into its place, without much difficulty. But the slit or rupture in the capsule remains open for a considerable time, and in many instances never closes. Under these circumstances, the patient is continually liable to a recurrence of the accident, and the slightest effort will sometimes be sufficient to induce it. It not unfrequently happens that the surgeon finds it impossible, by the most powerful extension and counter-extension to restore the head of the bone, even in the most recent cases. This is owing generally, as is now well understood, to the rent in the capsule being too small to admit the head of the os humeri to pass through and enter the glenoid cavity. When such difficulties exist, the surgeon discontinues the extending and counter-extending forces, and taking hold of the arm, uses it as a lever, and communicates a rotatory motion to it, the chief object of which is to tear up and enlarge the opening in the capsule. This being done, a very slight effort, in the way of extension, will probably be sufficient to reduce the bone.

When the head of the bone, instead of being restored immediately to its proper cavity, is suffered to remain in the armpit, for weeks or months, it will be found in a very different condition from that last described; inflammation takes place, adhesions form between the bone and surrounding parts, adventitious ligaments are created, a new socket is produced, the old one partially or entirely filled up, and the bone after a short time almost as firmly fixed as it was in its original position. Previous to the time of the enlightened and adventurous Dessault, such a case was deemed hopeless and irremediable. This great surgeon conceived the possibility of restoring the use of the arm, under these al-

most desperate circumstances, and succeeded in several cases of one, two, three and four months standing, by the following means. "Previously to making extension, it is necessary to move the bone *very forcibly* in every direction, in order first to break the adhesions, to tear the condensed cellular membrane, which serves as an accidental capsule, and to produce, so to speak, a second luxation, with a view to make way for a perfect reduction of the first. The straps being then applied, as in ordinary cases, serve the purpose of extension, for the accomplishment of which, the number of assistants must be increased. Oftentimes the first efforts are fruitless, and the luxated head remains stationary, amidst the *most violent efforts.* Let the extension then be discontinued: renew the *forcible motions* of the limb: carry the humerus upwards, downwards, forward and backward; *force the resistances* to give way; make the arm describe a large arch of a circle round the place which it occupies; let the rotatory motions on its own axis be impressed on it anew; and then re-commence the extensions, and let them be made in every direction. By these, the head, already disengaged by means of the preceding *violent motions*, will be brought to a level with the glenoid cavity, and ultimately replaced.*" From these extracts it will be seen, that Dessault strongly inculcates the employment of *forcible* and even *violent* exertions in the reduction of all old luxations of the os humeri; the success, indeed, which he met with, almost invariably, and that too in many instances, after other practitioners who employed milder means had failed, was such as to justify the practice completely, and induce other surgeons, both in Europe and in this country, to follow his example. The practice, therefore, has long since become general and established, so much so, that the surgeon who should refuse to attempt to relieve his patient, because the head of the bone had remained out of the socket several months, would be considered culpable by all intelligent members of the profession. Dr. Physick has, "in a variety

* Dessault's works, by Caldwell, p. 144.

of instances, succeeded after two and three months."* The late Dr. Dorsey, one of the best informed and most accomplished surgeons of this country, entirely approved of Dessault's practice, and followed it successfully in several cases. "Dr. M'Kenzie, of Baltimore, replaced a dislocated os humeri nearly six months after its luxation."† The same has subsequently been accomplished by Mr. Kirby,‡ of Dublin. For the last twelve or thirteen years, I have repeatedly reduced luxations at the shoulder, and some other joints, from two to four months standing, and although in several instances the adhesions surrounding the head of the bone and the new socket were so considerable as to require great force and extensive laceration, not the slightest accident has ever occurred. The records of surgery, indeed, furnish very few examples, so far as I am acquainted, of injury, much less of death, resulting from attempts to restore the head of the bone even after it had been displaced for a very long period. Dessault details the history of one case in which either a large emphysematous or bloody tumour formed under the pectoral muscle immediately after the head of the os humeri had been restored to its glenoid cavity. "Scarcely was the reduction accomplished, when a tumour rose suddenly under the pectoralis major, propagated itself towards the armpit and occupied immediately its whole extent. All the assistants, astonished at the phenomenon, knew not to what circumstance to attribute it. Dessault himself, a little embarrassed, thought first of an aneurism suddenly produced by the violence of the extension. The pulse of the patient, being scarcely perceptible in the side affected, and a syncope which supervened, appeared at first to favour this suspicion; but immediately the absence of a fluctuation, of a pulsation and of a change in the colour of the skin, the return of the pulse, the circumscription of the tumour, its resistance and the sound caused by striking on it, produced a belief that it was owing, not

* Dorsey's Elements of Surgery. Vol. I. p. 237. † Ibid.

‡ Kirby's Cases, with observations on Wryneck, the reduction of luxations of the shoulder, &c. p. 53.

to an effusion of blood, but to a disengagement of air that had been confined in the now lacerated cells of the cellular membrane. On the thirteenth day, the tumour was entirely gone. In the place which it had occupied a large ecchymosis appeared, produced, no doubt, by the rupture of the small vessels at the time of reduction."* This patient recovered perfectly in less than a month after the reduction, and no other similar case is mentioned, that I know of, either by Dessault or any other writer.

Although most writers on dislocations seem to think a rupture of the axillary artery, from attempts to restore the bone, after it has been displaced a few weeks, a possible occurrence, yet I have not been able to find, after very diligent research, a single instance of this description except one, which is merely glanced at by Mr. Charles Bell. "In this violent operation," says he, "one can imagine that if the axillary artery were at all diseased it might be torn; but I have not known of such an accident, though I have known such an ecchymosis succeed the operation of reduction, as would imply the rupture of some considerable vein. In employing the ambe in the New-Castle Infirmary, both the axillary artery and the muscles have been torn, so that they were obliged to amputate on the instant."† Mr. Bell is silent as respects the event of the case; there is every reason to conclude, however, that it could not have been otherwise than fatal. A very remarkable instance has been recorded by Loder of high inflammation, mortification and death, from an attempt to reduce a luxation of several months duration. "When Loder was studying at the Hotel Dieu at Rouen, a man came to the hospital, on account of some trifling complaint. The celebrated M. David, then the principal surgeon of that establishment, perceived that the patient had also a dislocation of the left arm. The displacement had already existed several months, and the limb had acquired some degree of mobility. M. David recom-

* Dessault's works, p 149.

† Bell's Operative Surgery. Vol. II. p. 247.

mended making a fresh trial to reduce the bone, and the patient's consent being obtained, the attempt was made with immence force, and the arm restored to its proper place again; but the event was most disastrous; for the whole limb was attacked with such violent pain and inflammation, that notwithstanding every means which surgery could suggest was immediately put in practice, mortification ensued, and the patient lost his life."*

The foregoing observations are calculated to exhibit the treatment of luxations of the os humeri as sanctioned and pursued by the best surgical authorities, and to show that the practice thus established, if not uniformly successful, has, with the exception of two or three cases, been unattended with danger. A question, however, may possibly arise—whether surgeons should be influenced by the event of the case I have detailed, and by those I have quoted, and deterred altogether from attempting reduction in dislocations of long standing, or whether the established practice should still be continued, unaffected by fortuitous circumstances or contingencies neither to be forseen nor controlled? To the latter proposition I have no hesitation to give unqualified assent, and to declare, that should a case similar in external appearance to that of James Scofield again occur, I shall feel justified in adopting a similar course.

Art. VI. *Case of Imperforate Anus.* By John T. Sharpless, M. D. of Philadelphia.

In January last I was called, during my tour of duty in the Dispensary, to see a female child, three weeks old, with imperforate anus, which had not been discovered till the infant was ten days old. On examination I found the situation of the anus closed by a thick dense membrane, the

* First lines of the Practice of Surgery, by S. Cooper. Vol. II. p. 466

fæces passing forward into the vagina, through an opening the size of a quill. By the probe I could discover that the rectum was quite enlarged to about half an inch of where the anus should be, and the stools having a free passage forward. The child from birth had been subject to great tenesmus, which perhaps arose from the tortuous course of the excrements.

In the presence of my friends Drs. J. K. Mitchell, and S. M. Fox, I introduced a small trochar much curved, through the opening into the vagina, and protruded the stilet through the closing membrane, which was exceedingly tough and hard, requiring great force to accomplish it. The opening thus formed was enlarged to the size of a goose quill by the bistoury, and a tent placed in, with directions to the parent to withdraw it as soon as an inclination to stool took place, which could always be foretold by the straining. This was done, but the opening was so small, and the fæces being determined forward by a ledge of flesh, little passed.

The object intended, was to dilate the opening by tents, but this plan was soon found unavailing, and with a bistoury I divided this ledge and enlarged the whole passage, forming an unobstructed outlet of the natural size.

I now introduced a piece of the largest stomach tube, two inches in length, and extending far above the opening into the vagina. This tube was wrapped with bougie plaister to a considerable size, opposite the forward passage, to prevent any matters going that way. This was withdrawn every day when no disposition to stool existed, and cleaned, and immediately returned. The bowels were kept laxative by castor oil, and the fæces all passed through the tube. The irritation of the foreign body soon subsided, the tenesmus disappeared, and in two months the opening through the recto-vaginal septum was closed. The tube was now left out several hours every day, so that any sphincter that might exist, should be called into action. In a short time a natural contraction seemed to take place; the edges of the wound became callous and cicatrized, and

in four months the child was perfectly well—presenting such an appearance that no person ignorant of the case, could, upon the most minute examination, discover that any malformation had ever existed.

Art. VII. ***Case illustrative of the utility of Charcoal in Constipation of the Bowels.*** Communicated in a letter to the Editor, by W. C. Daniell, M. D.

Savannah, June 18, 1823.

My dear Sir,

SINCE my communication to you, published in your Journal for November, 1822, upon the efficacy of charcoal in constipation of the bowels, I have had frequent opportunities of witnessing this disease, and of further testing the powers of the remedy.

This experience has not only confirmed all that I formerly stated as to the efficacy of the article, but has enabled me to add some facts, which I deem of sufficient importance, to beg you to place with those already recorded, for the purpose of imparting a full confidence in a remedy, for which I know no fair substitute. In its commencement, constipation is usually unattended by fever. In its early stages, acidity frequently prevails in the stomach. The secretions of the primæ viæ are chiefly of slime, which frequently is very tough and thick. The first evacuations are composed mostly of it. The charcoal unites intimately with this slime, forming balls varying in size, from half an inch to two or three inches in diameter. The duration of the disease appears to depend in a great degree upon the quantity of this slime, from its continued secretion. When the quantity is great, the balls formed with the charcoal are sometimes so large as to require some more active cathartics to expel them. This has frequently occurred with me. In one of the cases, the first evacuation was produced on

the evening of the fourth—in the other on the evening of the fifth day from the first exhibition of the charcoal, by the free use of castor oil. Where slime is daily discharged by the former, for more than four or five days, I have derived advantage from the blue pill, given intermediately, as an alterative, between the doses of charcoal. In no case has the cure been complete, where the charcoal has been discontinued whilst the stools exhibited any slime. In all the cases which I have observed, the fever, if it existed previously, has abated upon the use of this article, nor has it arisen to any extent, nor continued long during the exhibition of it. Much or violent pain is rare, whilst the charcoal is given, and when it does occur, it is not of long continuance. Occasional pains shooting through the bowels are all that is usually experienced, and sometimes these are wholly absent. Severe pain is sometimes felt in the loins, sacrum and hips. Thirst is generally constant—vomitings occasionally take place, and then the discharge is composed of a ropy slime with acid.

These vomitings I have never known to recur so frequently as to interfere with the administration of charcoal. In most instances, a dose of it immediately relieves this symptom. I have repeatedly met with cases where stools were not obtained earlier than the fifth day—and such is my confidence in the charcoal, that I would continue its use for a much longer period, if relief was not sooner afforded. I have now prescribed it in numerous cases—repeatedly in consultations, where every remedy commonly resorted to, had been tried in vain—and in no instance has it failed to realize my utmost expectations, with only one exception, and that was a case of relapse, attended with suppressed menstruation, from exposure to wet, which terminated in black vomit on the fourth day. In this case, constipation was rather an attendant on yellow fever than the primary disease: indeed, I never saw a better marked case of yellow fever than this was from the commencement. No one article was retained on the stomach for twenty minutes, from the commencement to the termination of the disease. It

occurred in September 1820. I had not then learned the controlling influence which potent rubefacients, long, extensively and repeatedly applied to the surface, frequently exercise over the stomach and bowels, and through these organs over the worst forms of our fever. I subjoin the following case to illustrate the treatment which I have habitually pursued for three years past in constipation.

February 24*th*, 1823.—Miss D——, a native of New York, arrived yesterday from Augusta. The weather was extremely severe. Her fatigue from travelling in the stage was great. Her frame is slender, and her constitution delicate—her age is about twenty-three years. I found her with fever, and considerable pain in the sacrum and neighbouring parts. Had a stool yesterday: tongue covered in the centre with a thin white coat.

℞ Sub murias. hydrarg. grs. xv.
Aloes socot. grs. jx.

M. divided in pill No. 7: one to be taken every hour.

Afternoon—the medicine had produced two free evacuations. Pain subsided. Complains of acid stomach.

℞ Carb. potassæ grs. vj.
Rhei. grs. xjj.
Aqua menth ℥j.
Aqua font. ℥ij.

M. one third to be given every hour.

25*th*.—No evacuation having been procured, ordered an infusion of senna and cream of tartar in broken doses. Has a slight fever without pain. The coat on the tongue extending and thickning. Afternoon—no stool having been procured, ordered

Sub murias. hydrarg. grs. xv. divided in pill No. 11, to be taken an hour apart, and worked off by the free use of castor oil.

26*th*.—The medicine had produced no evacuations by stool—the pulse somewhat full, equal and regular, with no pain. Ordered two table-spoons full of charcoal, to be given every half hour, and an occasional enema.

27*th*.—No pain—slight fever—had slept well—enemata

had brought away, once or twice, small quantities of charcoal without fœcal matter. Charcoal continued—occasionally an enema of forty grains of tart. emetic, disolved in a pint of gruel. The tongue has become swollen, and the edges tender and painful. Strong mercurial fœtor. Afternoon—some increase of fever and no pain. Upon rising, she vomited up some charcoal, and a tough ropy slime in considerable quantity. The enemata had returned unaccompanied with any matter whatever. Stomach tranquil. Had slept some. Enemata discontinued. Charcoal continued.

28*th*.—Had passed a good night—no stool—slight fever—occasional slight pains shooting through the intestines—charcoal continued. Afternoon—fever increased—venesection, ℥xjj.—charcoal continued.

March 1*st*.—Had slept composedly for a portion of last night. No stool. Coat on tongue disappearing from the edges and end—mercurial fœtor continues—but little thirst—blood drawn yesterday gives slight indication of inflammation. Charcoal continued. Afternoon—very slight exacerbation of fever. Charcoal directed to be continued until day light to-morrow, when a dose of castor oil shall be given, provided free evacuation be not sooner obtained, as the audible commotion in the bowels appears to promise as much.

2*d*.—Had voided a ball of black matter of the size of a musket ball, composed of charcoal and slime intimately blended—no fever and no pain. Directed another dose of oil, and afterwards charcoal to be continued as before, and occasional enemata to be given to assist the discharge of the large balls supposed to be in the lower intestines. Afternoon—had two copious stools, which were composed of hard balls varying in size from a musket ball to a hen's egg, and a heavy black coarse powder which lay in the bottom of the pot. The balls were found to be composed wholly of charcoal and mucus intimately mixed together, and so compact that some force was required to break them. The interior of them was hard and almost perfectly dry.

The patient becoming feeble, I ordered chicken water to be occasionally given. 10, P. M.—had passed another stool similar to the former—no fever and no pain—coat on the tongue disappearing—swelling of tongue subsiding—mercurial fœtor diminished—charcoal continued.

3d.—Had slept well—no evacuation since I last saw her. Directed oil and occasional doses of charcoal to be given. Afternoon—The oil had produced one stool of a similar character with the former—charcoal to be continued.

4th.—Has some fever—was told it had been high in the night, with occasional delirium. Directed a dose of oil, and afterwards charcoal. Afternoon—Having had no stool, the oil was repeated—less fever than in the morning—some pain in the sacrum and bowels—charcoal to be continued.

5th.—No fever—a free evacuation had been procured, which, on examination with my colleague, Dr. Screven, was found to consist of a tough, ropy mucus, in detached pieces, of several inches in length, and of water. The whole exhibited a dark greenish appearance, and was somewhat offensive.

I was now informed by her, that her physician in Augusta had expressed his belief of her having worms. Directed a dose of castor oil, to be followed by charcoal, as heretofore. Afternoon—Much the same as in the morning—no stool—oil and charcoal to be continued.

6th.—Had passed a stool, with much pain, consisting of four or five balls, the largest 1½ inches in diameter, slime and water. The balls were composed entirely of charcoal and slime—the slime similar to that of yesterday—slight fever—had slept some hours.

My patient is now very despondent. Directed the oil and charcoal to be alternated. Afternoon—Had passed a stool, very tenacious, and appears like jelly—the same medicine continued. 10 o'clock, P. M.—Has slept some—was now informed, that she, for the first time in her life, has the blind piles, to which she attributes the severe pains occasionally felt in the region of the rectum during this illness—had passed one stool similar to the former—charcoal

to be continued to-night, and early in the morning a dose of oil to be taken, and repeated every two hours, until my next visit.

7th.—Has passed three stools since three o'clock this morning, similar to those of the preceding day, with the addition of some lumps in one of them. The pain of piles had subsided—occasional pain in bowels—no fever—fur extending over the tongue—the oil and charcoal continued as heretofore. Afternoon—Has passed two stools similar to those of yesterday—charcoal to be continued through the night, and oil to be given early to-morrow.

8th.—Has passed two stools—one of them containing a ball of charcoal and mucus, full three inches in length, and two inches in diameter—the remainder chiefly mucus—no fever—coat on the tongue disappearing—occasional slight pain in the back and in sacrum. Had slept well the chief part of the night. Three grains of blue pill every two hours, and charcoal the intermediate hours, were ordered. Afternoon—Had one stool composed of mucus and balls—pulse soft and regular—medicine to be continued; and early in the morning a dose of oil.

9th.—Had enjoyed refreshing sleep—has passed three stools composed of balls and slime—charcoal and oil to be given alternately. Afternoon—Has passed one stool.—Ordered senna and cream tart. in broken doses—which being rejected, carbon was given. 10, P. M.—Had passed one stool. There was now a disposition to sleep, which she declared she had not before felt during her illness, although she had slept frequently.

10th.—The evacuation of this morning is without slime—her condition much improved—ordered a more nutritious diet—with a spoonful of charcoal to be given occasionally. Afternoon—Has passed another stool which is without slime.

11th.—Has some fever—has passed a stool composed chiefly of slime. Directed eight of the blue pills of three grains each, one to be given every hour. Afternoon—ordered a dose of oil to work off the pills.

12th.—The medicine has operated twice—the first stool has a considerable quantity of black granular matter at the bottom of the pot, and both stools very offensive—no fever—has not slept well—no pain. The blue pills to be given every hour, and worked off with oil. Tongue pretty clean—considerable appetite. Afternoon—The medicine has produced one free stool, which is very offensive, though with little slime—no fever—no pain—appetite still improving—charcoal to be given two or three times during the night. This treatment was pursued, with occasional omissions, until the 20th, when she was discharged. Her convalescence was slow, though regular.

I am, dear sir, very respectfully and sincerely, your obedient servant and friend, W. C. DANIELL.

ART. VIII. *Case of Pulmonary Disease, attended with some anomalous symptoms.* By SAMUEL JACKSON, M. D.

I WAS requested by Mr. E— to visit his daughter Harriet, aged seven years, on Thursday, April 5th. I found her labouring under the symptoms of cynanche laryngea or croup. Respiration was performed with difficulty, accompanied with a harsh whining sound—she had a frequent hoarse croaking, dry cough, recurring in severe paroxysms, during which the lips became purple, and the countenance livid: she was just recovering from a violent paroxysm of this kind as I entered the room. The skin was hot and dry, pulse frequent and tense. The fauces were inflamed, and the tonsils slightly enlarged. She was extremely restless, running from one part of the room to the other, in consequence of a constant feeling of suffocation.

I was informed by her mother, that on the Monday week, previous to my visit, Harriet had taken a cold, accompanied with a dry, harsh cough, for which antimonial wine and dul-

cified spirits of nitre were given. The cough had continued, though apparently getting better. When coughing she would complain of severe pain in the breast, but having good spirits, kept about as usual. On Wednesday the 4th April, she became suddenly very hoarse, so as to speak with difficulty, and the cough increased in intensity. During the night, the respiration became affected, and was attended, for the first time, with the croaking hoarseness, and the aggravated symptoms, I have described as presenting themselves to my examination.

I immediately opened a vein and drew twelve ounces of blood, in a large stream, while standing, which occasioned a tendency to syncope. The bleeding was succeeded by an emetic powder consisting of twelve grains of ipecacuanha, and two grains of calomel, and she was placed in a tub of warm water. The emetic acted very promptly, while in the bath. Instant relief was procured by this treatment, though my patient appeared much exhausted. She was put into bed, and a calomel purge directed to be given at noon. At my evening visit I found her in the same easy state: the purge had operated.

The next morning, Friday, 6th, she had recruited in strength, but was oppressed with phlegm, and the respiration embarrassed. Prescribed decoction of seneka snake root. At night she was very feeble and apparently sinking. Prescribed tincture of assafœtida, ammonia, and mint water.

Saturday morning—Much improved, but during the day her condition was extremely fluctuating—at one time oppressed, breathing with great labour, at another perfectly easy and composed. Saturday night—the difficulty of breathing had become permanent, oppression increased, and attended with great restlessness arising from a sense of suffocation. The coolness of the skin and weakness of the pulse counter-indicated depletion. But though my patient was in a very feeble state, I determined to exhibit an emetic, supporting her with cordials at the same time. The emetic operated kindly, and relieved the pressure of the symptoms, though the difficult respiration continued.

Sunday morning, 8th—Somewhat easier, but very low: the carb. of ammonia and decoction of seneka were administered. I observed, now, a very evident fulness about the throat, but could not prevail on my patient to permit me to examine the fauces. In the evening no doubt existed as to the tumefaction of the throat, which was considerable. On examination, a crepitation, as from a slight emphysema, was very evident. I could not inspect the fauces. From the symptoms that now began to make their appearance, and the total absence of pyrexia, which had not recurred since the first day of my visit, I was led to suspect that the distress and difficulty of breathing arose from the contents of an abscess, that had emptied its contents into the cellular membrane of the throat, producing pressure on the trachea. An appearance of pus having presented itself in an effort of coughing, determined me to attempt increasing its discharge by an emetic. With difficulty I administered a part of an emetic draught, which brought up a considerable quantity of mucus, evidently bronchial. Though the respiration became easier, the patient was extremely feeble, and to all appearance sinking. Wine and water were given to support her.

Monday morning, 9th—Contrary to my expectations, my patient was alive, and exhibited a little more vigour, though her respiration was extremely embarrassed. The emphysema was now so considerable that it had swelled the neck at least one-sixth of its circumference—it occupied a considerable part of the front of the thorax, rendering the mammæ very protuberant, and was slightly discernible in the face. The nature of the affection became apparent, and I was satisfied that unless the pressure on the trachea was speedily relieved, the operation of bronchotomy could alone prevent suffocation. I requested additional advise, and Dr. Parrish was called in. He concurred in the view of the case I had taken, and proposed to make an incision through the integuments at the most tumified part, in order to attempt procuring a discharge of the air and pus compressing the trachea by this means, previously to the performance of

bronchotomy. While preparing to effect this intention, a small quantity of pus was discharged by coughing. This circumstance indicated that the pus had found a passage, and pointed out the propriety of aiding its expulsion, through this channel. An emetic was again administered, which occasioned only a slight retching, but with the effect of evacuating, in the effort excited, a tea-spoonful of purulent matter. The respiration became more free, which induced a continuance of the plan of treatment. Our patient having become intractable, and refusing to take medicine. a sternutatory powder, composed of turpith mineral and liquorice powder, was blown up the nostrils, through a quill. The violent sneezing induced, was followed by a discharge of a table-spoonful of well maturated pus, with an immediate relief of all the distressful sensations under which our little patient had suffered. She fell into a deep sleep, which lasted some hours. During the afternoon, pus continued to be discharged in considerable quantities, by cough. The respiration was again oppressed towards evening. Wine whey and volatile alkali were given to support the system.

Tuesday, 10th.—The emphysema had increased very considerably. The face was bloated, and the eyelids so much distended as to close the eyes: it extended over the whole of the trunk, and could be perceived in the arms and thighs. Restlessness had returned: the breathing was difficult, but was relieved by discharges of pus, procured by means of the sternutatory: she was incapable of speaking with distinctness, and apparently suffering pain. Discontinued the stimulants.

Wednesday, 11th.—Hectic fever had supervened—skin hot—pulse quick and frequent—circumscribed flush in the cheek—emphysema at a stand—cough frequent, accompanied with expectoration of genuine pus: exhibited a cathartic, and confined the diet to light mucilaginous drinks.

Saturday, 14th.—Has been gradually improving—emphysema disappearing—discharge of pus lessened nearly one-half—cough very troublesome, but dry—pain in the left

side of the thorax—hectic fever continues, though less severe.

Monday, 16th.—Emphysema nearly gone—no pus discharged, though the cough continues—hectic remains, but light: antimonial powders prescribed, and farinaceous diet.

From this period, until the 25th of the month, convalescence advanced, when she appeared to be completely restored to health. On the 4th of May she was seized with a violent fit of coughing, which brought up about half a gill of pus. The cough continued, with occasional discharges of pus in small quantities, until the 8th May. Since that time she has enjoyed very good health, and been free from any pulmonary disease.

I am led to suspect, from the violence of the cough and pain in the breast, in the commencement of the attack, and from the extensive emphysema which subsequently appeared, that the disease, whose progress has been related in the preceding case, was originally a circumscribed inflammation of the superior lobe of one of the lungs, which terminated in a vomica—that the contents of the vomica, instead of escaping by the bronchiæ, must have found their way under the clavicle into the cellular membrane, giving rise to the emphysematous condition of the neck, face and trunk. The symptoms of cynanche laryngea, or croup, were probably occasioned by the irritation in the vicinity of the larynx, produced by the passage of the pus in that direction, being extended to that organ.

REVIEWS.

Art. IX. *Retrospective Review.* ΤΟΥ ΜΕΓΑΛΟΥ ἹΠΠΟΚΡΑΤΟΥΣ παντων των Ιατρων Κορυφαίυ, τα ευρισκομενα· Genevæ, MDCLVIII.

WITH the name of Hippocrates sentiments of admiration and respect will always be associated in the mind of the medical scholar. As the first who attempted to arrange the chaotic elements of medicine, and showed us that the right way to improve our profession was to observe the operations of nature—as the first concentrator of our knowledge in a system of rational practice, and as the most ancient depository of medical science, he deserves to be studied with attention, although his original features are nearly effaced by time—and accident or ignorance has robbed us of most of his genuine works. Enough, however, remains to cause regret that more is not to be found. We can still select, from the writings attributed to him, a sufficient number of his observations to convince us of the superior character of his mind, and to satisfy us that the title affixed to his name could not, with justice, be any thing less than that, which has been appropriated by common consent—"*the father of medicine.*"

In addition to the merit we discover, or fancy we discover, in an author who wrote so long since, in a state of society so different from our own, we are prejudiced in his favour still farther by those feelings which are excited by the examination of every subject of high antiquity. Distance, while it renders objects indistinct, veils defects from our sight, and leaves free scope to our imagination, whose delight it is to envelop the subjects of our contemplation with the most inviting drapery, and adorn them with the most fascinating colours. To the harsh and rugged mass

it imparts softly flowing outlines, and to the uncouth and disjointed, lends the gracefulness of ease, combined with the chastest proportion. We need not, therefore, be surprised to find some persons so devotedly attached to whatever has the semblance of emanating from Hippocrates, as to perceive in his works no imperfections, and believe that they contain the germ of every subsequent improvement. So blinded are some by their zeal as to lavish commendations on every fragment attributed to their idol, without troubling themselves to inquire whether the whole may not possibly be spurious, or at least miserably corrupted by interpolation or by transcription.

When speaking of Hippocrates, physicians do not seem generally aware, that *seven* persons, belonging to the same family, and successively bearing the same name, have lived and cultivated the science of medicine in Greece. But the one most distinguished, who has received the highest honours, and to whom *all* the writings are commonly referred, is HIPPOCRATES THE SECOND, son of Heraclides of Cos, and grandson to Hippocrates the First. He was born in the first year of the twenty-fourth Olympiad, 460 years before the Christian era. He attained his greatest celebrity in the twenty-sixth Olympiad, 436 A. C., and died in the one hundred and second Olympiad, 375 A. C., being 2198 years ago.

Of the other physicians of this family we know little more than their names. Some of them acquired reputation as practitioners. Of Hippocrates the Third, it is known that he became a disciple of Plato, and composed several books on medicine, one of which, at least, we shall hereafter find among those attributed to his grandfather; Hippocrates the Second.

We have hinted at the corruption and uncertain authenticity of the Hippocratic writings, which will not appear singular when we recollect the numerous causes of vitiation to which they were exposed. At the time of their composition the use of the papyrus had not become general, nor was this substance fabricated in sufficient quantity to be

extensively employed—hence the writings of Hippocrates were originally inscribed on tablets covered with wax,* or were traced on the prepared skins of animals.† In the first case the preservation of copies must have been attended with much difficulty, and the inscription exposed to innumerable accidents. As the only mode of multiplying copies was by transcription, so laborious a task was entrusted to professed drudges, by which free access was given to every variety of error. When these works were transcribed by physicians, they were often more seriously injured by interpolations, or substitutions, made with a view to sustain some favourite doctrine, by the apparent sanction of the highest authority.

When Hippocrates flourished, the characters employed in writing were the capital letters (ΤΑΓΡΑΜΜΑΤΑΜΑΚΡΑ) without the intervention of spaces or points. It was not until the ninth century of the christian era that the small letters and contracts, (τα γραμμα τα μικρα ϗ συσταλμενα,) were introduced. The latter were employed to facilitate transcription, and the mistakes attributable to the employment of them will account for the great diversity of the manuscripts written since they were invented.

Many of these writings being intended by the author as notes for his own use, were not prepared for public inspection. This, in addition to the antiquity and obsoleteness of many of the words used by Hippocrates, will account for the difficulty and obscurity so apparent in many of the books which are best authenticated. In a sketch intended for his own use, a few words would be sufficient to recall all that he required, while it must necessarily be barely intelligible to others. These difficulties were considered sufficiently great in the time of Erotian to require a glossary of superannuated expressions, and another was made by Galen, for a similar reason. These difficulties increase with the advance of time, as it is evident that many words sufficiently well known in the days of Galen are now obsolete,

* δελτοι.

† διφτεραι.

which permits the commentator to wander freely—and when he cannot decide on the precise meaning, from proper authority, to give such an explanation as suits his own views or accords with his own purpose.

A very fruitful source of fraud and deterioration was opened by the kings of Egypt, who founded libraries in imitation of Aristotle, and offered great rewards for ancient manuscripts, to multiply copies of which they expressly prohibited the exportation of papyrus. Crowds of adventurers were eager to profit by their enthusiasm, and impose on their credulity—and being well aware that they were anxious to have richer libraries than the kings of Pergamus, feared not that any thing offered would be scrupulously examined or rejected.* They either sold the writings of the lesser Hippocrates as authentic works of Hippocrates the Great, or made additions to the genuine writings, in order to gain more money by their increased extent. Not a few of these interpolations may even now be distinguished from the authentic, by their gross errors and anachronisms, although considerable pains were taken to make them resemble the original. Other persons sold the genuine writings of Hippocrates, corrected and amended by themselves—while some presented any fragment they could obtain, as his compositions. Ship masters were ordered to collect manuscripts wherever they went, which still farther tended to increase the impositions practised.

The learned men of Alexandria tasked themselves with the selection of the genuine writings, and carefully separated such as they believed authentic from the spurious.* As these were inscribed on a particular tablet, the supposed genuine writings were called "writings of the little tablet," *τα εκ του μικρου πινακιδιυ*. Nevertheless, the Alexandrian selection is found to be very far from correct, and the authenticity of particular treatises no better established than before.

Uncertainty as to the genuineness of the Hippocratic writings has existed from remote antiquity. The evil was

* Galen.

not as great in the days of Galen as at present, inasmuch as he could examine manuscripts copied at an earlier date, and nearer to the time in which Hippocrates wrote. Yet, though he could procure the most ancient and perfect manuscripts, he was not able to add any very positive testimony of their authenticity. On the correctness of his conclusions we principally rely, although, as he is not always consistent with himself, we should not yield ourselves to him implicitly.

The Hippocratic writings were, in the first instance, mutilated by his sons, Thessalus and Dracon, and by his son-in-law, Polybus, who belonged to the dogmatic school. Not content with changing the order of the treatises, they made many interpolations, added treatises of their own composition, and endeavoured to explain obscurities by substituting their own notions. The greatest mutilators of the works of Hippocrates were Artemidorus Capitor, and his relative, Dioscorides, who lived during the reign of the emperor Hadrian. They not only replaced obsolete expressions, but made interpolations, or struck out boldly whatever displeased them.

As these writings have been thus abused, it is very desirable that we should have some test by which to decide on their genuineness. Yet, on examination, such a test is scarcely to be hoped for. Hippocrates, though a Dorian by birth, wrote in the Ionic dialect, which was for a long time the most prevalent. Atticisms are occasionally met with in his writings, but not sufficiently often to render an exception to the above statement necessary. By some, especially the French critics, the dialect has been thought a good test of authenticity, without, however, proper foundation. Certainly nothing could be easier for a fraudulent interpolator than to imitate a dialect sufficiently common in Greece, which continued to be used as late as the time of Lucian, and which differed from other dialects in orthography rather than in construction.

The condensed, forcible, and peculiarly *laconic* style of Hippocrates, was thought by Galen, to be most distinctive of his genuine works—and, indeed, to offer the only un-

equivocal evidence of authenticity. This *laconicism* seems considerably to have arisen from necessity, as from scarcity of writing materials he was obliged to employ as few words as possible. In making notes for his own use, he might write thus from choice, for the reasons before given. It is true, that this laconic style may also have been counterfeited, though with much less ease than the dialect could be, as it is much harder to imitate modes of thought and peculiarities of expression, than to observe particular rules of orthography or syntax.

We cannot hesitate to reject as spurious, those treatises which contain palpable anachronisms, as the reference to discoveries made in anatomy, long after the time of Hippocrates, or the introduction of opinions first broached by persons who were not born while he lived. Such is the mention of the valves of the heart, first discovered by Erasistratus,* and of the Platonic and Peripatetic philosophy, which flourished after he ceased to be.

As Hippocrates was the first who separated medicine from philosophy, some have thought that those works are most genuine which are freest from philosophical speculations. It is not correct, however, that in separating medicine from philosophy, Hippocrates rejects the latter entirely. Although his precepts and practice are deduced from observation, he felt the want of general rules too sensibly, not to aim at discovering general principles. His philosophy differed from the philosophy of his age, in this, that he theorized from *facts*, while others dealt exclusively in speculation. Yet it is acknowledged that the books which are most philosophical are the most doubtful. In consequence of devoting himself so much to observation, the Empirics claimed him as the founder of their sect, though without justice: in the same way the Dogmatists claimed for themselves a similar honour, because Hippocrates examined attentively the diseases of particular organs.

Some of the Hippocratic writings are evidently repetitions of others, with occasional additions of sentences or

* Vid. Kurt Sprengel.

words. It is probable that these treatises have been collected by admirers of this great physician, who have added their own remarks, or introduced fragments attributed to him. The principles in many of these writings are evidently in accord with those of Hippocrates, though the details can scarcely be from his pen, while in many instances the repetitions are too frequent, to permit the belief that he sent them forth so slightly altered under a new title.

The number of commentators on the Hippocratic writings has been very great, and offers a proof of the esteem in which they have always been held during twenty centuries. But from Artemidorus Capitor, who lived under Hadrian, down to De Mercy, who wrote in 1815, these commentators have all taken the liberty of altering or supplying words, whenever it seemed desirable to them. This they justify by a slight reason, a loose analogy or a forced construction, or else offer it on their own authority. Even in modern times, the text has been altered to suit particular purposes, and to support modern doctrines, while commentators have occasionally changed the entire sense of the original, because they fancied the expression unworthy so great a man. De Mercy does not scruple to change the text in some instances, because the observations made contradict *his experience*, as if the phenomena of fever were so immutable in the mode of their appearance, as to be precisely the same now in France, as they were nearly three thousand years ago in Greece. Nor does he consider the manuscripts themselves as of sufficient authority to decide on certain points, and is guided by the more modern manuscript when it is better suited to his notions, than the most ancient, which, however, was copied in the twelfth century of the Christian era.

Of all the editions published of the Hippocratic writings, that of Fœsius is the best, being very free from inaccuracy, and what is better, from prejudice. Entertaining a high veneration for his author, he does not, however, carry it to the excess of supposing him infallible, or to prevent his distinguishing falsehood from truth. The text as given by Fœsius,

is acknowledged by all the critics to be the most correct, and his Latin version the most *faithful*—that is, he is not so solicitous for the universality and prescience of his author as to suit his translation to the existing state of science, but gives his meaning as originally expressed.

The translation edited by *Haller*, in the selection called "Artis Medicæ Principes," is in general very correct, though in conciseness and perspicuity, it is not equal to that of Fœsius. De Gorter also, in his excellent Medicina Hippocratica, has done justice to his author, and to his own high reputation.*

A very convenient and portable edition of Hippocrates has been published at Paris by M. De Mercy, before referred to, the text being accompanied by a Latin and French version. In some respects we cannot altogether agree with this gentleman, especially when he corrects the text from his own experience, and when he asserts that the terse laconicism of the original may be better rendered in French than in Latin. As the two versions go together, this is not likely to cause much error, as he has to paraphrase in French, to express direct enunciations of the original, which are similar in the Latin. He merits praise for his zeal and perseverance in trying to extend a taste for medical literature, and for diffusing the works of the founder of medical science.

To judge from all the evidence before us, we must agree that the Aphorisms, Prognostics, and second book of the Predictions, have the strongest claim to our respect, as genuine works of the Coan sage. There is so much excellence of observation, and beauty of expression throughout these, as to convince us that they could only emanate from a sedulous observer of nature, endowed with a mind of the highest powers. He is occasionably chargeable with exalting particular facts into general principles, or reasoning too

* De Gorter was also the author of a work, entitled "Chirurgia Repurgata," which contains views of the pathology of inflammation, very closely resembling the doctrine most generally received on this subject at present.

extensively from limited experience—but this evil is not of frequent occurrence, or productive of much injury. In these works may be found numerous instances of that energetic and comprehensive brevity which Galen thought so peculiar to Hippocrates, though a style not very dissimilar is employed by Herodotus, who was a cotemporary. These works alone, would be sufficient to secure his title to immortality, as they are not only possessed of the beauties of expression, but they are mostly based on the immutability of truth. The imperfections are few, compared with the excellencies, and many of these may have been introduced during the lapse of time, and should not be universally charged to the author.

It is not our intention at this time, to present any analyses of these works, which has been very ably done by Galen, Therculiaris, Daniel Le Clerc, De Gorter, K. Sprengel, Hecker, and mány others, but shall content ourselves with referring the reader to these sources. We cannot, however, avoid expressing our regret at that severe censure passed on Hippocrates by the great lord Bacon, who, however qualified as a philosopher to judge of the correctness of his reasonings, was scarcely sufficiently aware of the difficulties attending the arrangement of medicine, in any thing like system, to pronounce with fairness on the value of this great physician's labours. That Hippocrates elevated experience too much, and that he gave great importance to things of small account, may be conceded. This, however, should not cause us to forget that he delivered medicine from the shackles in which it was held by superstitious ignorance and juggling priestcraft, as well as from the subtleties of the dialectics—that it was Hippocrates who first taught physicians to look to nature for wisdom, and showed that the way to understand the treatment of diseases was to study their phenomena at the bedside of the sick.

When looking back on the few who have gained the most imperishable reputation, which is equally raised above accident or decay, we shall uniformly find that such fame has been solely awarded to works of *truth* and *usefulness*. While

the theorists who dazzled the profession for a time, and misled their less considerate brethren, have expired and sunk into oblivion, the humble and unostentatious seeker after truth, by cautiously studying facts, has daily risen in esteem, and become the admiration of posterity. Hippocrates, Harvey, Sydenham, Jenner, and those who resemble them, may be truly called immortal, as their reputation is founded on the unchangeableness of nature and the wants of their fellow creatures. Such examples ought to produce imitators, seeing that the same reward continues to be offered to such as are willing to labour in the right way.

A principal object of this retrospection is to call the attention of the profession to the uncertain authenticity of writings, which it is daily becoming more fashionable to quote in sanction of recent opinions. Where a fact is adduced, which is sustained by subsequent observation, or a result mentioned which is of uniform occurrence, no objection can be urged against using the name of Hippocrates. But when a mere dictum, or unqualified and unsustained assertion, is employed to prejudice the reader in favour of notions equally crude, we should recollect that the name of Hippocrates has for a long time represented a shadow, whose very outline must be undetermined, as the body by which it was cast is so imperfectly known.

Θ.

Art. X. *Della Natura delle Febbri e Dei Metodi di Curarle. Con alcune doduzioni sulla natura della convulsioni, sulla estinzione delle febbri contagiose; sull'uso delle immersioni fredde e calde; sulla esistenza, ed indale della complicazione morbosa; e sulla relativa modificazione da introdursi nell' indicazione curativa.* Del Dottore G. Giannini, Medico ordinario della Imperiale Real Corte in Milano. Tome due, pp. 396 & 476. Seconda Edizione. Napoli, 1818.

Desirous of imparting to our readers some portion of the valuable information contained in the recent medical

writers of Italy, to which they have little access, we presented in our last number, a concise notice of a tract on intermittent fever, with a promise of following it up by a further account of a more elaborate treatise on the same subject, by the celebrated Giannini. The work before us, however, embraces very copious disquisitions of great merit on several other subjects, and particularly on contagious fevers, convulsions, gout, rheumatism—as well as on cold immersions—the existence and nature of morbid complication, and the relative modifications to be introduced into our curative indications. We shall, at present, confine our remarks to the section of the work which treats of intermittents, as most immediately interesting, from their wide prevalence, of late, among us—and as engrossing as much space as we can conveniently appropriate to a single article. We may hereafter recur to the work, and complete our review of its valuable contents.

As preliminary to the consideration of intermittent fever, our author gives the history of the external and internal use of cold water and ice in fevers—particularly those of a contagious nature. He cites the practice of Samoilowitz, in the plague at Moscow, which consisted in the free use of ice applied to the surface by way of friction. In many cases, when the patients appeared to be at the last extremity, they have been suddenly revived by this means. We are told of a girl, sixteen years of age, who, seized with the plague, seemed on the second day to be beyond the reach of all remedial measures:—extraordinary debility, paleness and shrinking of the surface, and continued stupor, threatened an immediate extinction of life. The fæces, menses, and urine were all discharged simultaneously. The body was now directed to be rubbed with ice, while the neck and face were simply wet with cold water.

Scarcely was this operation, which lasted an hour, completed, when the whole surface became of a lively red, and vapours arose from it, as from a person who has just come out of a bath.

Tremors and chilliness followed this state. The patient

was dried and put to bed. At two o'clock, four hours after the first frictions, the symptoms above enumerated returned, and the frictions were repeated. At ten o'clock, similar symptoms required similar means, and the frictions with ice were used for the third time.

On the third day of the disease, things were nearly stationary: the frictions as above, used four times. Rice water acidulated with lemon-juice, was the only article given by the mouth.

On the fourth day of the disease there was some amelioration. The bubo began to rise. The frictions were repeated four times. The only drink was cool water, with a little elixir of vitriol.

On the fifth day the patient still better—bubo larger—fever and all other symptoms milder—frictions used three times. Rice acidulated, taken frequently.

On the sixth day she could sit up in her bed, and spoke with facility. Three frictions.

On the seventh day, took two drams of bark every half hour. At mid-day and evening used the frictions.

On the eighth day—dryness of the skin, and hardness of the pulse—(owing, perhaps, to the bark); frictionsr enewed by means of cloths wet with cold water.

On the ninth day the patient was again better, and on the sixteenth entirely restored to health.

Another case, with petechiæ and carbuncles, and other of the worst symptoms, was cured on the seventh day by a similar treatment to that just mentioned.

Every medical reader knows the success of De Hahn in the treatment of the epidemic fever of a malignant character which prevailed in Breslaw, in 1737, by means of the external use of cold water. De Hahn himself recovered by this practice, which was eminently successful when contrasted with that of the other physicians who would not make use of the remedy.

In his letters on Egypt, Savary informs us that in the burning fevers to which the inhabitants of the Saïd are subject, the remedies consist of water used largely as a beverage, bathing in the river Nile, and attention to regimen. He

relates the case of a sea captain, who, seized with the plague at Constantinople, stripped himself naked, went up on deck, and passed the night in that state, exposed to a heavy dew, which penetrated to *his bones*, as he himself expressed it. In the morning his fever and agitation were calmed, and the use of a sea bath was the only additional curative means to accomplish his complete recovery.

Bruce, in his travels, speaks of the violent fevers which reign at Mesuah, and which generally terminate in death on the third day. If the patient survive to the fifth day, he is often saved by making him drink water, and nothing else, and by throwing over his body a quantity of cold water, even in his bed in which he lies, without being dried, until he is deluged by a second shower.

Morandi, a Venetian physician, tells us of some sailors at Constantinople, who, attacked by the plague, threw themselves into the sea, while in a state of delirium, whence they were taken out cured.

Desgenettes relates similar instances of cures among the French soldiers, who rushed into the Nile—and when besieging Jaffa, into the sea.

As respects the use of cold water internally, the credit of the application to the cure of fevers, is due to the Italians. For some details respecting this practice, our readers are referred to a former number of this Journal,* in which the subject is incidentally noticed in the article on the new Italian doctrine of counterstimulus. If we are to credit some historical accounts of the uses of cold water as a remedy, we should trace it to the Spaniards, by whom it was taught to the Neapolitans. *Fra Bernardo Maria di Castrogiaanna*, a Sicilian friar, who performed such wonders at Malta, in 1724, with cold water, was the pupil of an Arragonese, named Rovida, who, twenty-four years before had introduced the practice into Naples. Fra Bernardo acquired a great reputation at Malta, as one who cured, or pretended to cure, by the above means alone, violent palpitations, scirrhous tumours of the liver, cephalagia, colics, diarrhœa,

* Vol. II.

hæmorrhages, malignant fevers, dropsy, rheumatisms, nephritis, small-pox, and syphilis, sciatica, the gout, &c. No wonder a good remedy should fall into disrepute, when enthusiasm or charlatanry lead to its misapplication and abuse. This man, it appears, attacked indiscriminately, diseases of all kinds by iced water, either taken internally or applied to the surface by friction, or introduced by *enemata.* There was, however, some method in his treatment. Some patients were allowed to eat freely; others were restrained to the most strict regimen. The *Capuchin remedy* was soon known in France, but appears to have never had a fair trial, and the ridicule thrown on the practice by Le Sage, subjected it to the fate of those frivolities which are ever under the influence of fashion in that country. Pomme, however, in his *Traité des affections vaporeuses des deux sexes,* &c., published in 1760, considers cold and ice water as a powerful agent in a variety of circumstances, among others, in fevers *not humoral,* and which display a spasmodic character.

We have already spoken of De Hahn's practice in the Silesian epidemic. His friend Theden informs us that this skilful physician made a still more extensive use of the remedy than had been previously imagined. It was by his advice that Theden applied cold water and ice with such success to strangulated hernia; and in a case of violent inflammation of the foot. "Emboldened by such trials," continues this writer, "I employed the remedy externally in small-pox and malignant fevers. I employed it on occasions when there was absolutely no longer any hope, when every person despaired of any resource; sometimes it was useless; it often accomplished miracles." We are apprehensive that these, like many other medical miracles, must rather serve as beacons to warn us against dangers, than as guides to direct our course. Many facts are cited which came under the observation both of Theden and De Hahn, attesting the efficacy of the practice in mania. Examples are also given of hypochondriasis and other chronic affections cured by the abundant use of cold water, taken daily, and persevered in

for a long time. Theden, in his "New Observations," attributed his then advanced age of eighty years, principally to the use of twenty to twenty-four pints of water daily, a practice which he had persevered in for forty years. When between thirty and forty years of age, he was hypochondriac in the extreme, and a prey to the darkest melancholy: he suffered from palpitations of the heart, and indigestion, and thought he could not survive six months. But from the time when he began to drink water, all these symptoms vanished, and he was healthier at an advanced period of life than he had been at an early age, and entirely free from hypochondriasis. He was himself a proof also of the efficacy of cold water applied externally, in a violent inflammation and tumefaction of the fore arm, extending to the elbow joint and arm, with acute pain and fever, in consequence of a wound of the finger in opening a fistulous deposite at the anus. The symptoms were so alarming as to determine him to submit to amputation, when, remembering the good effects of cold water, he had recourse to the use of it, and was cured.

We are indebted to the English writers for some of the earliest and most detailed accounts of the use of water as a prophylactic and remedial mean. John Hancock, a clergyman, published as far back as 1722, his *Febrifugum Magnum*, or *Common Water the best Cure of Fevers*. His attention was directed to the subject from his suffering under a violent fever, with cough and jaundice, in which he was given over by his physicians. He had, however, the good fortune to survive; but on the following spring the disease returned with the same violence. A friend recommended a little pulverized amber, in a quart of cold water; the cough was moderated. But it soon returned, and, suspecting that the amber had no share in his former relief, he drank a pint of pure water, which produced the same good effect as the first prescription. Having retired to bed, he drank a glass of water after his first sleep, passed a tranquil night, and had a slight perspiration in the morning. Finding himself better, he continued his simple beverage until the fourth day,

and was freed from his fever, cough, and jaundice. This is more than the most sanguine *mercurialist*, *chlorinist*, or lover of dandelion could promise himself from the use of his favourite remedy; and is one among the many proofs that the precise epoch when the patient is given over by his attending physician, is that most favourable for the successful exhibition of new remedies, either by the enthusiastic regular, or the cabalistic charlatan; or, what is often the same thing, for showing the powers of nature to advantage. Hancock, encouraged by the success attending the use of cold water as a beverage on himself, next tried it in the diseases of his children, as small-pox and measles. To a little daughter, apparently on the point of death, from the repelled eruption of measles, her breast exhibiting only livid spots, he gave at first a small glass full of cold water, being doubtful of the propriety of his plan; and in two minutes after another—then a third, and a fourth. After the administration of the third glass the eruption re-appeared, was red and elevated, as usual; the respiration, which had been laborious in the extreme, now became easy; and, after taking the fourth glass, she fell into a sound sleep of four hours duration. She soon recovered her health.

A Treatise on the Medicinal Virtues of Common Water, by Smith, published about this time, was a summary of the most interesting notices on the subject, particularly of what had been written by the physicians of England, who were partisans of the cold water regimen. He cites, among others, the *Treatise on Cold Baths*, by *Floyer*, and that on the *Cures performed by Cold Baths*, by Dr. Brown.

In 1721, when the attention of the French was particularly excited, and all their fears and sympathies enlisted by the ravages of the plague at Marseilles, Geoffroy (Stephen Francis) defended a Thesis before the Medical Faculty of Paris, in which he lays down the problem: *Whether Water be an excellent Preservative in time of Plague*. After rejecting all other means as useless, he concludes in the affirmative. About the same period, Hecquet, examining the ques-

tion, whether the sick ought to be debarred the use of drinks, considers water an universal specific.

The later communications of Wright, Gregory, Jackson, M'Lean, Brandreth, Gerard, and, above all, the work of Currie, have shown to demonstration the safety and utility of the external use of cold water, in a variety of febrile diseases, and ought to be sufficient to dispel any prejudice, remaining against a remedy ever at hand, indicated by nature herself, and so delightfully soothing to the poor sufferer in the hot stage of a fever, that none but those who have felt the transition from torments equal to those by poets fabled, to the sweet slumber of fairy bowers, can adequately appreciate its powerful influence. The writer of this article has felt it, and may be excused, therefore, for the warmth of the present eulogium. It is much to be regretted that the external use of cold water in fevers should, like every new remedy, or old one newly revived, be extolled at first as a panacea, used without a discriminating knowledge of its effects, or a judicious selection of cases, then be neglected, undervalued, and declaimed against as inert or injurious. We are sorry to add, that the imprudence of young surgeons in the British colonies, particularly in the East Indies, has contributed much to the disparagement of our remedy, and to giving force to the prejudices of the vulgar against it. A desire to call anew the attention of our medical brethren to a means of relief, prompted by instinctive feeling, explained by theory, and sanctioned by reason and experience, will be deemed a sufficient motive for our dilating rather more on this subject than is consistent with the nature and limits of a review; at the same that it will be our apology for having introduced a variety of authorities not referred to by our author. On the same grounds, we should consider ourselves justifiable in dwelling on the internal use of water, cold, tepid, and warm; citing, in our support, the names of Saunders and Physick, and the well known case of the celebrated John Hunter, who enjoyed repose by a draught of warm water, when the usual anodynes failed to produce their wonted effect; but as a work which embraces the history of bath-

ing, and the analysis and medical use of all the known mineral waters, has been completed by us some months ago, we shall pass on to a consideration of the cases and train of reasoning adduced by our author, in support of the use of cold water in fevers, and in illustration of his pathological opinions. In the performance of this task we shall better consult our own inclinations, and act, we are sure, more in conformity with the feelings of the great majority of our readers, by giving, as far as in our power, an analysis of the work before us, than, *petulanti splene cachinno*, indulging in flippancy of remark or bitterness of invective, for the gratification of personal vanity or private resentment.

As one of the physicians to the large hospital at Milan, our author had ample means of testing the accuracy of received medical opinions, and the relative success of different modes of practice. His attention seems to have been more particularly directed to the subject of fevers, the different symptoms of which, and the practice in them, especially the external use of cold water, he discusses with an accuracy and precision, which may be truly called analytical, and which, to use the favourite phrase of our French brethren, "*ne laisse rien à desirer*." The ravages going on simultaneously in so many parts of the United States during the last two seasons, render fevers a subject at this time peculiarly interesting, and the blending of endemic and epidemic influence, and other morbid complications, predispose us to an attentive hearing of all that can be said from any and every quarter.

We may iterate what our author so forcibly says, "that fevers in their effects and mode of propagation are the true permanent plague of Europe; that they sweep off too great a proportion of the population, in the eyes of humanity and political economy; and that in Milan alone, there die some thousands annually, victims of fevers."

One of his prefatory remarks, is well calculated to fix our attention, viz: that in his observations, he had not in view either authors or opinions of the schools or parties,—but consulted nature, and sought truth. He protests with

great reason against the system of sweeping generalization of Brown, who proscribed the study of the complication of diseases—a knowledge of which is absolutely indispensable, as well for correct prognosis as for successful therapeutica. Genius may at once arrive at results, without following or tracing the intermediate steps, but the imitation of such boldness by common minds must eventuate in disappointment and misfortune; neither Newton in his closet describing what must be the precise figure of the earth, nor Rush at the bed-side of his patient reading all the varied characters of disease, can in a few brief calculations and expressions lay down the rule and the law for the great body of natural philosophers or physicians. In our profession above all others, "patience of thought," so much insisted on by the illustrious Englishman just cited, is the prerequisite to success: not that patience which consists in plodding and imitative dullness, whereby its possessor approximates as closely to the inertia of matter, after the laspe of half a century, as at the beginning of his career; but that which is displayed in careful, cumulative observations, followed by systematic arrangement, at once to store the memory and strengthen the judgment.

The tendency of the medical inquiries of the day is decidedly analytical: guided by physiology and pathology we trace the radiations of disease from the diffused external symptoms to the central and deranged viscus: acknowledging the different modifications of vitality and susceptibilities of the various organs, we select those remedial agents for which they may seem to have an affinity or peculiar appetency; and no longer believe that the mere states of high and low, will afford sufficiently clear indications of diseased action, in its multifarious grades and proteiform concealments. The French school, enlightened by Bichat and Broussais, has led the van of the physiological and pathological division; to our own must be awarded the merit of the bold employment, and nice discrimination of the agents in the therapeutical phalanx. In the work before us, we find much of the Italian character for patience of research,

and ingenuity in explaining the different phenomena of nature, whether animate or not, without the author's being in this instance led away, like many of his countrymen, into abstract speculation or ingenious subtleties. Though he has furnished facts and arguments in favour of the Rasorian or counter-stimulant doctrine, he does not seem to have framed observations or introduced cases for the support of it, or any other. That our readers may judge for themselves we proceed to give them a sketch of the work, and in so doing, shall follow the author chapter by chapter.

Of the first we have already given the outlines in the history of the use of cold water in fevers: it concludes by a notice of the prejudices among the members of the profession, as well as the vulgar, against this practice; and an inquiry into how far these prejudices are well founded, with an acknowledgment of the difficulty, and even danger of eradicating them. The idea of cold's being a stimulant, advanced by Brown, and sustained by Currie, helped to prolong our ignorance of the nature of the remedy, and consequently of the selection of cases to which it was peculiarly applicable. The simple removal of a morbid stimulus, or diminution of excitement, was mistaken for a new and healthy stimulus, and this plain and simple reasoning overlooked, that "if heat be a stimulus, and if this stimulus be necessary to us, it is not every grade of it which will suffice to preserve us in the state of health. If the heat be excessive, the health will be disturbed, sometimes to such a degree, that we are rendered incapable of performing the ordinary functions of life. In this case the abstraction of the excessive heat, which is the same thing as the application of cold, will restore the health and remove the debility. And hence cold, or the abstraction of heat, may seem to be corroborant."

In allusion to the prejudices against cold affusions in fever, as against all new remedies, our author very properly remarks, that "what is new is in opposition to the self-love of the many." "We seek for a firm footing on the old road," says Zimmerman, "for fear of being drawn on to the new."

"We love better that others should rave with us," says Tissot, "than that they should show that we rave." But an apology for opposition is found in this instance in the well known facts, that a heated and inflamed part suddenly exposed to cold, becomes much worse, and alarming symptoms often supervene; that humidity is often a cause of rheumatism: and finally in history we read of the fatal consequences of large draughts of cold water, or immersion in it, when the body is over heated. When the soldiers of Alexander arrived on the banks of the Oxus and drank to excess of its gelid water, so many of them lost their lives, that in the language of Quintus Curtius *multo major horum numerus fuit, quàm ullo amiserat prælio:* and when that prince, on his entrance into Tarsus, at the head of his troops, covered with dust and sweat, stripped in their presence and jumped into the cool Cydnus, *vixque ingressus subito horrore artus rigere cœperunt: pallor deinde suffusus est, et totum propemodum corpus vitalis calor reliquit. Expiranti similem Ministri manu excipiunt, nec satis compotem mentis in tabernaculum deferunt.* (Q. Curt. lib. 3, cap. 5.)

The second chapter opens with the author's reasons for his first describing the nature and treatment of intermittent fever, in preference to the synthetical plan pursued by Currie, who, "the only one among the writers already cited, that undertook by experiments to penetrate still farther into the nature of fevers, began with considering the contagious ones. He consequently began on complicated data. He applied a remedy, with the action of which he was ignorant, to a disease which he knew still less. He did not commence with the simple and the known, to pass to the compound and the unknown. No wonder then, that his conclusions corresponded exactly with his premises; that he ended by not knowing either the remedy or the disease; or that he believed that the stimulus of the one did nothing but remove the spasm of the other."

After enumerating the different modes of applying cold water, he gives the preference to that by immersion, which, though not dissimilar in its effects from the others, has the

advantage of resembling more nearly a common bath, shocking the feelings of the patient less, and affording less handle for prying malignity.

Cases confirmatory of the success of cold immersion are next given. We select the following out of a larger number.

"*Case* 1.—A young man of a robust habit, twenty-eight years of age, after two paroxysms of tertian fever, with the cold, hot and sweating stages, came to the hospital, where in the period of the hot fit of the third paroxysm, he presented face and eyes red, anxiety, preternatural heat, thirst, pain of the head—pulse ninety four. In this state he went into the cold bath. A minute afterwards, every morbid symptom had disappeared. After five minutes, the coldness of the water becoming unpleasant, he returned to his bed with a feeling of the most perfect health. The pulse was seventy-three, respiration natural, skin cool, thirst gone, pain of the head entirely vanished. He took immediately two drachms of bark, six drams in all, before the hour for the fourth paroxysm, which did not take place. He continued the bark for some days, and had no more return of fever."

Cases 2, 3, 4, *and* 5, of quotidian fever, were similarly treated with equal success. Case 5, was of a young man, twenty-one years of age, in the fifth day of the fever: he had constantly vomited the bark. After the cold immersion he could retain this febrifuge, and on the following day had no febrile paroxysms. He was soon restored to health.

"*Case* 8.—A man thirty years of age, came to the hospital on the 24th of October, with a regular tertain fever, of which he had had already four paroxysms. He had been purged freely, and had taken the bark without any effect, so that he protested to me, that he would rather die than take any more. During the period of the fifth paroxysm, I found him with the usual symptoms, pain in the head, anxiety, very strong pulsations in the carotids, acrid heat, nausea, restlessness. He had, in addition, the abdomen exceedingly hard, somewhat swelled, and constipated bowels for three days preceding. I had him immersed immediately in cold water, in which, at the expiration of two minutes, he said he had been restored to life. All the symptoms, except the hardness of the abdomen, had disappeared. After twelve minutes, he returned to bed full of vivacity. I prescribed for him six drachms of the greater bistort and gentian.

"25th.—Apyretic.

"26th.—At nine o'clock, before the usual hour, he had a fe-

brile paroxysm, but without a chill. The immersion was repeated. Medicine as before.

"27th.—Apyretic. He had several watery stools. The abdomen soft.

"28th.—Apyretic. Two grains of opium were united to the gentian and bistort. The fever did not return."

Assured of the efficacy of cold immersions in simple intermittent fevers, our author was next desirous of testing its utility in that variety of the disease, usually termed *pernicious fever*, the hemitriteon of the Greeks, malignant tertian of Cleghorn, and semi-tertian of Senac and Fordyce.

"*Case* 9.—A Frenchman of about thirty years of age, had a paroxysm of fever with cold, heat and sweat, which from the account given me seems to have been very severe. I prescribed for him large doses of bark, and as he complained of a disposition to vomit and incipient diarrhœa, some grains of opium were united with it, and an enema of diascordium was ordered. Twenty hours after the first paroxysm, he had another very violent one. I found him with a burning skin, extremely furious and delirious, so that it was difficult to keep him in bed. He had vomited a considerable portion of the bark on the first coming on of the fit, which began with a chill of very short duration, He had an insatiable thirst, with parched mouth, inflamed eyes, and a most acute pain in the superior part of the orbit. It was impossible for me to count the pulsations at the wrist during the hot stage. The sweat appeared two hours afterwards. Fearing that the vomiting and diarrhœa would render the use of the bark unsuccessful, recourse was had to the cold bath, and large doses of bark combined with opium were then continued both by mouth and enemata."

"Ten hours after the second paroxysm he was seized with a third. After a very slight chill, which was not perceptible to the touch, the hot stage came on, accompanied by the customary raging and delirium, and the other symptoms above mentioned, so as to render it necessary to have him held by two strong men—though struggling between the most violent fits of vomiting. I chose one of the short intervals of exemption and had him immersed in a cold bath. In a minute after, he began to eructate very much. The vomiting, pain of the eyes, the thirst and restlessness ceased, and the greatest calmness was immediately restored, together with the greatest clearness of ideas. By the aid of the bark, which was thenceforward retained, and a second cold immersion on the supervening of a simple increase of heat, the disease was completely eradicated."

Case 10, was a man of a robust and athletic temperament, 44 years of age: the symptoms were of the most alarming

nature—delirium and the greatest prostration of strength—bark was prescribed. The type of the fever now became so obscure that no intermission was observable. The cold immersion was had recourse to, which the patient himself prolonged nine minutes, with the best effects. The use of the bark was persevered in, and the fever disappeared.

Case 11.—In a young man of a robust habit, 26 years of of age, the dry and burning skin, subsultus tendinum, pulsations 130 in a minute, and other symptoms, left it doubtful whether the fever was a continued nervous one or a malignant intermittent. Viewing it as one of the latter description, the cold immersion was had recourse to, with the usual relief ensuing—pulse 108. During the two following days, the immersions and the bark, with opium, were continued, and on the third, the man was convalescent.

"Not only then," continues the author, "do common intermittents, but the pernicious, and the most alarming of the pernicious, present the same phenomena, and display the same effects from the action of cold immersion."

"I begin then by laying down, and holding as demonstrated, the following general primary proposition: *cold immersion, employed in the period of the hot stage of intermittent fevers, puts an end immediately to the paroxysm.*"

It next remained to be determined whether the immersion was sufficient to cure the fever, as well as stop the paroxysm.

Cases 12 and 13—the first of tertian, the second of quotidian fevers, were treated at first by immersion, exclusively with a view to ascertain this point; and it was found that the cold immersions, though they *stopped the paroxysms*, were inadequate to prevent their *recurrence*, which end was finally accomplished by the use of the bark, conjoined with the cold bath.

Case 14 had been treated with bark for a quartan of three months duration, without success. Immersion alone was next had recourse to, and repeated at suitable epochs, from the 9th of December to the 1st of January; but the fever still continuing, bark was again resorted to, and the patient was definitively cured.

In *Case* 15, of a young countrywoman, with quotidian

fever, the immersion alone was first used, for seven days successively, without preventing the recurrence of the paroxysms, though their violence was abated. The bark, used in addition to the remedy just indicated, cured the disease in three days.

"Before we proceed further, let us, then, in virtue of what has been detailed, establish the following propositions:

"*It appears that the use of the cold immersion exclusively, in intermittent fevers, is not sufficient to cure them radically; and,*

"*It is certain that the use of the bark, following immediately that of the immersion, renders the effects of the latter permanent.*

"*Cold immersion is the remedy for the paroxysm; bark for intermission. The former cuts short the fever; the latter prevents its return.*"

"If the cold immersion be the remedy of the paroxysm, and the bark that of the intermission, it results, that when we know the mode of action of the immersion and that of the bark, we are equally well acquainted with the nature of the paroxysm and that of the intermission."

Our author, dissatisfied alike with the assertion of Brown, that the cold, hot, and sweating stages are only different degrees of debility; and with the opinion of Currie, that cold affusion acted as a stimulus, conceives it indispensably required that the different periods of an intermittent fever should be subjected to an exact and minute analysis.

If, continues he, the face of the patient, before flushed, become pale in the time of immersion; the strong pulse converted into a feeble one; the feeble almost cease to be perceptible; these effects are not those of exciting causes. If the cold immersion were exciting, it ought, employed in the cold stage, to produce salutary effects. But so far from this being the fact, the experience of Currie himself shows that it may be speedily fatal. If the cold affusion were, according to the doctrine of this writer, corroborant, it ought, when employed in the period of convalescence, to accelerate it; but here again his own experience is opposed to his

theory. If the cold immersion were corroborant, it might, when employed in the period of the intermission, prove a substitute for the bark. But it has been demonstrated, and it will be still more clearly, as we advance, that it only produces good effects in the last stage of the fever. "I advance with confidence also the following argument. A hot immersion is stimulant, then a cold immersion is debilitating."

But a cold immersion (reply some) sudden and abrupt, produces a shock. It would be desirable to have the meaning of this word shock accurately defined. "I prefer, however, to oppose theories by facts, and to adduce some of the latter, in which cold acted without suddenness or shock."

Gregory, Brandreth, De Hahn, have obtained effects similar to those from cold immersion, by simple sponging of the surface of the body, lightly, and at different points in succession. Our author had a person in the hot stage of a tertian fever, put into a bath, 98° of Fahrenheit (of a temperature equal to that of the patient) which was gradually cooled by the introduction of cold water. In about half an hour the bath was reduced to the temperature of common cold water. In a few minutes after the coldness of the bath began to be felt, but the febrile symptoms, the pain of the head, thirst, restlessness had disappeared, before such coldness was perceptible, that is to say, without shock. The patient was put to bed, took the bark, and was restored to perfect health two days afterwards. Since then, the simple abstraction of caloric, without any shock, cuts short the fever by debilitating; so the cold immersion, which equally cuts short the fever, does it by debililating.

It has been demanded, whether the strong impression produced on the patient by the cold immersion is not, in part, the effect of fear? Delicate and timid persons are, indeed, much troubled on the occasion, and hence a kind of shock. It will, however, hardly be asserted, that this shock, the effect of fear, is a stimulus, since we know that this emotion principally acts by diminishing the impetus of the circulation of the blood.

The application of the principles of hydraulics to the vi-

tal movements, and particularly to the circulation, and explanations given, and objections thence brought, on the effects of cold water externally applied, are successfully combated by our author. The consequence of such notions is the practice so generally adopted of wetting the head before entering into the cold bath, in order, we are told, to prevent the great and sudden afflux of blood to the head, and the consequent risk of apoplexy, or of hæmoptysis, from a similar determination to the lungs. We strangely forget that the first action of the cold bath is that of immediately diminishing the force of the heart's contractions, and thence of debilitating the whole arterial and venous systems. This effect is constant, whether it be from nervous action on the heart, in cases of sudden immersion, or from the abstraction of caloric, which debilitates, and diminishes the volume of the blood.

The very important practical bearing which these respective opinions exert, will be our apology for giving somewhat in detail the reasonings of the author.

"If a true regurgitation of blood took place, from the external to the internal parts, from the periphery to the centre, the pulse ought to increase in strength and fulness. The heart, oppressed by the unusual quantity of blood, would redouble its efforts to eject it; and the larger avenues, as the carotids and the temporals, which are not in the bath, the arteries of the wrist, which I have often kept out of the bath, and which, relatively to the vessels of the surface, ought to be considered as internal parts, or at least as parts which always act in unison with the heart, and give the just measure of its movements and strength, ought to swell and become more vibrating. So far from this, their movements, like those of the heart, are immediately rendered feebler, and their fulness diminished. The action of the cold bath—the action of the cold immersion is, then, that of debilitating.

"If the repercussive power of cold were established, how could the brain of a fœtus resist, when the mother enters a cold bath, as is the custom in many countries? If it were true, apoplexies would be of frequent occurrence in those young persons who throw themselves into cold water, for the amusement of swimming. Out of so many febrile patients, whom I have had immersed in the cold bath, with eyes and face flushed, pain of the head, and strong pulsations of the carotid and temporal ar-

teries, no one sunk apoplectic. In fact, not reasoning from the laws of hydraulics, nor fearing the pretended alarming plethora by revulsion, I have uniformly commenced the immersion by the lower extremities. The blood then retiring gradually from these extremities, as the immersion advanced, and carried, according to this hydraulic theory, to the head and face, ought to produce a tumefaction and reddening of this latter, which is not at all the case."

"Hæmoptysis is one of those diseases in which the cold bath is contra-indicated. Marcard and almost all the systematic writers on medicine, warn us to abstain from the cold bath, when there is a spitting of blood and any other pulmonary derangement which may make us fear it. Such a caution is not certainly founded on experience, since no one was bold enough to prescribe the cold bath in hæmoptysis. It is founded on the hydraulic theory, which, false in other respects, is equally so in hæmoptysis. The truth is, that this disease has not a better remedy than the cold bath, as I shall have occasion to show in its place.* The same may be said of other hæmorrhages, and particularly the uterine."

"The theory of the regurgitation of blood by the action of the cold bath is then a chimera. If in the cold bath, there is a real constriction of vessels at the surface, the same constriction takes place simultaneously in the interior."

"In persons whom I directed to immerse the legs and part of the thighs in cold water, I observed that, at the moment of immersion, the pulse at the wrist was sensibly diminished in frequency. The contractions of the heart were equally diminished in strength. In others, on whose heads I had the water poured, the same thing occurred in the pulse and heart. How explain these phenomena, if the action of the cold immersion were hydraulic?" *pp.* 67, 69.

Darwin cites two cases; one of hæmorrhage from the kidneys, the other from the nose, cured by partial immersion in cold water, and subjoins the following, "Query, might not the cold bath instantly stop hæmorrhages from the lungs in inflammatory cases?—for the shortness of breath of those, who go suddenly into cold water, is not owing to the accumulation of blood in the lungs, but to the quiescence of the pulmonary capillaries from association." (*Sect*. xxvii. i. 1.)

* "I speak of hæmoptysis in paroxysms, accompanied by sthenic symptoms, and not of the simple habitual spitting of blood, constituting the hæmophthisical pulmonary habit."

From these facts our author infers, that "*the cold immersion, in cutting short the paroxysm of an intermittent fever, acts by debilitating.*"

"The cure of intermittents, such as we have described it, is performed by two remedies of an opposite nature, by the bark which strengthens in the intermission, and by the cold immersion which debilitates in the paroxysm. The period of the intermission and that of the paroxysm are then two states of an opposite nature."

But in what does this diversity between the paroxysm and the intermission consist.

"That the period of intermission is a true debility, the happy, constant and ordinary effect of bark, and of almost every other stimulant, and the infelicity of the opposite method of treatment, seem to place beyond all doubt. That the period of the hot stage is a state strictly inflammatory, is what, denied by Brown, insisted on by others anterior to him, it seems can be only decided by a more minute inquiry."

The continuance of the inflammatory diathesis in the phlegmasiæ, as in peripneumony, measles, catarrhal affections, and its ephemeral duration in the hot fit of an intermittent fever, constitute the first difference.

In the true inflammatory affections, there is no period which indicates a truce and then a return of all the symptoms. The inflammatory appearances of the hot stage of an intermittent fever, are periodical.--Second Difference.

"Peripneumony, and the measles, are cured by venesection. The period of the hot fit of intermittents does not admit of this remedy.--Third Difference."

Though we may admit the propriety of this difference in general, yet we must dissent from our author, when he asserts that venesection is inadmissible in the hot stage of an intermittent fever. One of the first cases of this disease, which fell under our care, of a young man who had taken both bark and arsenic without effect, was very promptly cured after a pretty copious bleeding during the hot stage. We were on that occasion influenced by the opinion of Senac, who recommends venesection among other remedies, and asserts that he has seen double tertians converted into simple tertians by this means. The authority of Cleghorn goes to

support this practice: he preferred opening a vein in the beginning of the hot fit; by which means the sick were immediately relieved; *the immoderate heat of the body (which is often productive of fatal effects) was diminished;* and the critical sweats were brought on sooner, and in greater abundance. By thus recognizing similarity though not identity of action between bathing and bleeding in the hot stage of fever, we are justified in placing them on the same footing in the cold stage of the disease, and reprobating the employment of either in such a case, notwithstanding the opinion hazarded by Doctor Rush, that "bleeding is certainly safe during that coldness of the body which takes place in violent fevers." We remember to have seen a respectable physician in Virginia, who very nearly fell a victim to this practice, directed by himself, in the cold fit of an intermittent fever. Whenever it is attempted with a view to rouse the dormant susceptibility of the system, we should at the same moment be prepared to pour down ether, laudanum, and other diffusible stimuli, if the reaction be not immediate.--To recur now to the reasonings of our author.

"Inflammatory diseases of a milder description, as coryza and colds, if they do not require bleeding, demand at least a proportionably debilitating or antiphlogistic treatment, and this treatment must be continued throughout the whole course of the disease without alternations of remedies of an opposite nature. In the period of the hot stage of intermittents, besides the slightly antiphlogistic method, such as is required in the milder inflammatory diseases, we cannot however remove it, or prevent thereby its return. The alternate use of remedies of an opposite character is necessary—Fourth Difference."

There is no time, when heating remedies are admissible in true inflammatory diseases—Opium is contra-indicated in them.—The duration of the hot fit of intermittents may, on the authority of Lind, be ameliorated and shortened by laudanum, as we shall indicate and explain in the course of this work.—Fifth Difference.

The causes which produce inflammatory diseases belong to the class of stimulating agents. The cause of intermit-

tents, and consequently of the stage of which they chiefly consist, are debilitating. Purgative remedies sometimes produce a relapse.—Sixth Difference.

The position on which our author grounds this last difference is Brunonian, and was deemed one of its strongest; but we now acknowledge that agents of a debilitating nature will give rise to diseases avowedly inflammatory.

Whatever may be the termination of inflammatory diseases, even though it be the most favourable, as by resolution, it never takes place but in a certain space of time which cannot be less than a few days. The period of the hot fit commonly ends in a few hours. Nor from a repetition of the paroxysms, does the hot stage ever produce any of those results which are common in inflammatory affections.—Seventh difference.

The inference from this is, that

"*The period of the hot fit of intermittent fever is not a state equivalent to that of inflammatory diseases properly so called.*"

If, as Brown asserted, the hot stage consists in debility, how can it be entirely removed by cold immersion, a debilitant? It is not a partial alleviation of symptoms, but an entire solution of the disease thus produced. We may then establish the following proposition.

"*The period of the hot fit of intermittent fevers, which is entirely terminated by cold immersions, cannot on this very account consist in a state of debility.*"

"If the period of the hot fit does not consist in a state of debility; if it does not consist in an inflammatory state; it only remains to qualify more precisely its nature, and to substitute a positive definition to that which hitherto in the propositions laid down, has only been negatively expressed. And here I have to indicate a new mode of action of the living fibre."

After some preliminary reasoning, in which we regret not having the time to follow our author, he goes on to say, that the debility, peculiar to the cold fit, lasts during the whole of the hot fit. "The inflammation then (if I may yet for a while call it so,) which is developed in the midst

of debility, subsists without dissipating this latter, subsists with it, as by it it is originated and supported." Then

"*In the period of the hot fit of intermittent fevers, the living fibre is affected with debility and excessive excitement at the same time.*"

The laws of excitement, established by Brown, are now referred to, and a history of the cause and symptoms of the cold stage of intermittents given. "It is precisely at this period, that from the action, at other times innoxious and salutary, of the most simple and habitual stimuli, we look for the most extraordinary reaction of the same fibre, on the very account of its having become weak and excitable. This is the last stage. Already has the warmth of the bed and of the fire, by its stimulus, so much the more grateful, inasmuch as it directly remedies the unpleasant sensation of cold, restored the first degree of vital energy." But the effect of caloric and its stimulus, are no longer salutary. Excessive and unnatural excitement follows, which may be either checked at once by cold immersion, or is resolved by the supervention of perspiration.

"I have said, that in virtue of one of the most incontrovertible canons of Brown, the last only took place in consequence of the cold stage, and that the one was a necessary consequence of the other." On the same principle that the eye is affected after being some time in darkness and then suddenly exposed to light; the half starved man who greedily swallows meat and drink, and the person depressed by grief who suddenly receives joyful news.

The non-occurrence, at times, of the cold stage, in intermittent fever, diminishes the force of our author's reasoning, though we admit its validity in general.

"The last stage left to itself, in the natural course of the fever, terminates slowly by sweat, and is instantly cut short by cold immersion. We may now observe that this effect could not be hoped for from any other agent or remedy than the cold immersion—and this is in virtue of the above theory. The hot stage being, as it were, a compound of *permanent* debility, and of *temporary* excessive excitement, any thing which tends to di-

minish this latter too much, *and beyond the immediate necessity* would be hurtful."

"Having removed by the cold immersion the morbid re-action, and diminished the momentary excessive excitement, the use of the bark may be afterwards attended with immediate success, thoug it hcannot be tolerated, in general, in the hot fit."

This follows from the premises laid down. The re-action being spent, and the disease simplified by cold immersion, the indication to be fulfilled is simple—to remove the existing debility, which is best accomplished by the bark. Some explanations are given, to show why this vegetable tonic and stimulus should not bring back the former symptoms of excitement and fever. Sensibility varies greatly during the whole course of the paroxysm—least at the commencement of the cold stage—greatest at the period between it and the last stage, and this is precisely that of the greatest debility. In those cases where no cold fit comes on, the greatest debility is at the beginning of the last fit. The former is not absolutely necessary for the appearance of the latter, since the debility, peculiar to an intermittent fever, may attain a considerable height without any cold symptoms. The alternations of heat and cold, of which a patient sometimes complains in intermittent fever, proceed from the different degrees of sensibility in the different parts of the body—some running more promptly into the last degree of debility; others of a less delicate structure are slower in arriving at the same point. This cause, joined to the different distances of various parts from the centre, that is, from the heart, the distributor both of blood and caloric, accounts for the gradual change from the cold to the hot stage. There are, however, some parts of the human frame which tolerate, with almost entire impunity, a want of caloric, and others again, which, by their situation, structure, and importance of functions, cannot be deprived of a small quantity without the most marked and disagreeable sensation of cold. The stomach is, in this latter case, a viscus, which, from the experiments of Spallanzani, and the observations of other physiologists, demands much caloric.

Giannini frequently found that in cold immersion, when the water touched the scrobiculus cordis, the sensation of cold was insupportable to some, who made no complaints when their extremities, and even the lower part of the abdomen, were immersed. Hence we infer, that there may be a sense of cold without any cold really existing at the time. That which occurs in intermittent fevers may be thus explained.

"The debility constituting the period of cold, has already retarded the circulation of the blood. The aorta no longer propels with force into the extreme capillaries, while the latter, seized with debility, are almost without action; they do not pulsate, and barely transmit the blood. By this means, there is not a total want, but a diminution of blood—those which ramify on the internal surface of the stomach, predisposed, perhaps, by particular circumstances, to particular debility, deprive it of the caloric especially necessary for this viscus; hence the universal sensation of cold. But the extremities of the aorta, which ramify in the breast, the head, the loins, are not in a similar state. The caloric does not abandon them with equal facility—and, for cold to be felt in these parts, a greater abstraction of heat is necessary, than from the stomach. The debility must be augmented, and then the cold will extend to these parts, but the stomach, at this time, will be in the beginning of the hot stage, owing to its greater relative sensibility."

Our author next proceeds to an explanation of the phenomena attending the sweating stage, based on the principles already laid down.

"The period of sweating is destined to accomplish that slowly, which art performs in an instant, by means of the cold bath." At the commencement of this stage, there being no important evacuation either of sweat or of the caloric, in excess, the excitement cannot differ materially from that which prevailed in the height of the hot stage, nor can there be any greater degree of debility. From these premises, our author infers, that the coming on of sweat in intermittent fever, does not contra-indicate the cold immersion, but, on the contrary, that he has used it in such cases with decided benefit.

We must confess that we consider this practice as hazard-

ous, if not perilous, and would prefer foregoing the probable advantages to the risk of unpleasant consequences, that must follow a want of tact, in not seizing the precise moment when the sweating stage is not too far advanced.

The intermission succeeds the sweating, owing to the diminished sensibility from this latter process. The debility increases insensibly during this period, and prepares the way for another paroxysm.

Such is the brief transcript of our author's notions on intermittent fevers—the consideration of which, he closes by an inquiry into their causes. Cullen attributed them principally to marsh miasmata. Brown, to all those powers which tend in any manner to debilitate.

"The opinion of Cullen is more accredited than proved. That of Brown, in a general sense, the most correct, is, as every body must see, too general:—he does not specify the true and final *modus operandi* of these powers, so that they should produce precisely the intermittent fever, and no other disease. The fact is, we see debilitating powers cause dyspepsia, diarrhœa, colic, gout, hysteria, and not intermittents. We have known of men and animals perish from inanition, by forced fastings, without any febrile symptom in their pulse, or previous paroxysm of intermittent."

That a residence in a marshy country is a cause of intermittent fever, explains nothing. We know not how these marshes generate the miasm—nor in what this miasm consists. How does it act? How can it produce the febrile cold? are questions asked by our author—and as he says, may be extended to the other debilitating causes. Their mode of action is not sufficiently specified, nor determined in such a manner as to show us how they are calculated to excite exclusively the intermittent form of fever.

Previous to advancing his own sentiment, he gives us the history of a very important case, which we think worthy of being presented to our readers in his own words.

Case 16.—"A man of about 50 years of age, of a robust habit, in using a catheter for a difficulty in urinating, irritated the urethra, a little beyond the prostate gland, which was followed by most acute pain, and the discharge of a few drops of blood. A few hours after, he was seized with a chill—then heat, and

the symptoms peculiar to this stage, finally sweating, succeeded by the intermission. He had about ten paroxysms of the same nature, which were only overcome by the bark.

"Will it be said, that the almost immediate supervention of the fever, on the local injury of the urethra, was fortuitous? Two years after, while using the catheter as before, and introducing it to the same point, by which he experienced similar most acute pain, with the loss of a little blood, he had the same form of fever, with chill, heat, and sweat. About eight paroxysms succeeded each other, with periods of intermission of various length, which were, as on the former occasion, entirely overcome by the bark.

"Three years afterwards, this person had occasion to use the catheter again—fresh irritation of the urethra—fresh pain; renewed paroxysm of fever with chill, heat, and sweat, which recurred three times, and which was again overcome by no other remedy than the bark.

"Little benefited by experience, while using some time afterwards the catheter in his usual awkward manner, he was seized with so violent a pain of the urethra, that the fever which succeeded was accompanied by the symptoms of one truly *pernicious*. Violent vomiting, diarrhœa, insupportable pain in the head, legs, and knees, delirium. I prescribed for him a drachm of bark every hour, which his stomach rejected. I united opium with it, and was constrained to carry the dose of the former to ten grains, daily. But I was hardly able with it to obtain a slight mitigation of the disease. Spirituous drinks, the anodyne liquor, and generous wine, were not more successful. I succeeded best with the anti-emetic draught of Riverius, and was able, by this means, to make the stomach retain enormous doses of bark, which at every minute were on the point of being expelled. By degrees, after eighteen paroxysms, the violence of the symptoms began to abate, the periods of intermission to be prolonged, and the patient having swallowed extraordinary doses of bark and opium, was perfectly cured."

Our author infers, with great reason, that the local irritation of the urethra was the true cause of the fever—that a mechanical body thus thrust against a very sensible part of the urethra produced a lesion of the nerves, which was extended to the whole nervous system.* "There may, per-

* In confirmation of the great and peculiar influence which the urethra exerts on the nervous system, he gives the case of a man afflicted with old gonorrhœa, to whom he recommended the introduction of two opium pills into the urethra, as far as the fossa navicularis, and to retain them there during the night. "He not only slept better than usual, but preserved

haps, have pre-existed in the patient a decided predisposition for many years to intermittent fever. It is still not the less true, that a local irritation evolved an universal disease, only curable by general remedies."

"Let us establish, then, the following proposition, which flows immediately from the case just given.

"*A local lesion of the nerves, with much sensibility, may produce an intermittent fever.*"

It is not known, nor is it important to inquire, how the local lesion affects the system at large. But, ignorant of the cause, we may inquire into the effect, with some advantage; that is, into the nature of the general affection, and in what it consists.

The fact of the influence of the nerves in the production of caloric, does not seem to have received those pathological applications of which it is so susceptible. Chemistry has, in part, explained to us the origin of caloric, and its subsequent distribution through the body, by means of the pulmonary apparatus and circulatory system generally. But its conservation is due, in a great measure, to the nerves.

"The man, in a state of the greatest cold, if seized with an emotion of anger, suddenly, without the lungs being able to respire even once, has his face flushed, his veins turgid, and skin warm. The cold of night and wintry snow does not deprive lovers of their caloric—the nerves preserve it for them. We see in our improvvisatori the eyes sparkle, the jugulars swell, the muscles act almost involuntarily; and to all these phenomena, accompanied by manifest signs of increased caloric generally, sweat often succeeds. All is due to the nerves. On the other hand, melancholy intelligence, the narration of an atrocious deed, spread a chill over those members which just before were the warmest. The action of the nerves has dissipated the calo-

nearly the whole day an invincible drowsiness, an obstuse pain of the head, and a continued desire to vomit. On expressing my surprise to the patient, he replied, that he had experienced similar effects from opium taken by the mouth—a remedy his stomach could not tolerate. After a lapse of six days, I persuaded him to repeat the experiment—the same results—and I observed similar results a short time afterwards in another person, who, by my advice, introduced the same quantity of opium into the urethra."

ric. Fear has the same effect, where the exertions to avoid it do not produce a counteracting influence.

"Will it be replied that this is but a pure play of sensation? But in fear, the loss of caloric is real and sensible to the touch. In the movements of anger, and in other exciting nervous emotions, the augmentation of caloric is also sensible to the touch."

Nor can it be said that this is a simple translation of caloric from the internal to the external parts—for the caloric could not be carried to the latter without abandoning the former, and in so doing leave a sensation of cold, which does not take place. The pulsations of the heart and large arteries are increased in strength and frequency during the heating, as it may be termed, of the external parts, and show, of course, that the internal ones are not destitute of heat. Some maniacs, by moral causes, that is to say, by the action of the nerves, resist cold for a long time, and support the longest fasts.

Plausible as are the observations of the author on the generation of heat by nervous influence, we are far from considering his position as established, and are compelled to acknowledge that much mystery still hangs over it.*

Our author next proceeds to inquire how the nerves have the faculty of alternately giving or taking away caloric from the system, of producing a real *increase* or *diminution* of *quantity*. Admitting that the caloric pre-exists in the system, he establishes the position, "*that the heat of the living bodies is in direct ratio with the motion of its vehicle*," and this vehicle is the circulatory system. The analogy of other bodies purely physical containing latent heat is not, we think, very happily introduced to illustrate the evolution of caloric in the human frame, and we pass on to the supposition that the rapidity of the circulation, neither necessary for the purposes of nutrition and secretion, is for the evolution of heat.

It may then be laid down as a general maxim, in our author's opinion, that every thing which impedes the evolution

* For an interesting inquiry into the causes of Animal Heat, see a paper by Dr. Harlan, in the Philadelphia Medical and Physical Journal. No. 5

of caloric in the mass of blood, is calculated equally to produce an intermittent fever, and that the only condition necessary is, either that the cause which impedes this evolution of caloric be very active, or that there pre-existed in the patient a considerable degree of debility. On these principles we can conceive how intense passion in robust persons, or slighter in those of a weaker habit, may produce intermittent fever. But for the morbid effect to take place, it must be precisely those nerves debilitated, which influence directly the motion of the arteries.

"In fact, in those places which, as we shall soon see, by their humidity or noxious exhalations, are known to occasion intermittent fevers, the effects do not take place except in the case of the patient sleeping a night in them, which is as much as to say, that the nervous system must be in a state of inaction, or indeed of passiveness, as it is during sleep. Hence wakefulness is recommended to nocturnal travellers, and the juice of the grape to the inhabitants of moist and marshy places.

"And if causes equally debilitating may produce, and do daily produce dropsies, diarrhœas, hæmorrhages, vomiting, scurvy, scrofula, rickets, marasmus, hypochondriasis, and not intermittent fevers, a convenient explanation is found in the nervous system not being directly affected in some of these diseases; or, if affected, in the arterial system not being brought into action. In others, the debilitating causes acted either slightly, or too slowly, or partially, in some particular point or organ of the body; and in others finally there did not exist previous debility, or predisposition to facilitate the effect.

"I now pass on to miasmata. Are paludal or marsh miasmata, the ordinary cause of intermittent fevers, as is the opinion of Cullen, and as result from an infinite number of observations made before and since his time?"

But previous to replying to this question, an inquiry is instituted, whether such miasmata really exist. Our author inclines to the negative, and supports his opinion by the following reasons.

When a morbid matter, inimical to the animal economy, is introduced into the system, it is wont to give notice of its presence by eruptions and spots on the skin, which does not happen in intermittent fevers.

The morbific matter working on the system, is generally expelled by evacuations, called critical, and more usually by

sweat. But the sweating in intermittents is the effect of the hot stage, and not an expulsion of the miasm. In fact, if the hot stage be cut short by cold immersions, the sweating does not take place.

Morbid matters, inimical to the human frame, once introduced, or simply applied, have the power of reproduction and multiplication, and of generating a disease in individuals similar to that which had attacked others, with whom the former had been in contact. There is nothing of this in intermittent fevers. If there were really miasm introduced into the system, it would be difficult to conceive how the fever could be cut short after one paroxysm, as is often the case, contrary to the well known nature of contagious diseases, which run a certain course.

It would be a gratuitous supposition to say, that the bark and cold immersion neutralize the miasm.

"The diseases which are the consequence of extraneous matters introduced into the system, are all communicated by contact, as I shall have occasion to show. The air is not a vehicle of any contagious disease. It is even the decomposer of all contagions. Thence by analogy also, of miasm.

"If there existed a paludal miasm, its effect ought to be specific and uniform, as is petechial fever, which produces its like; the variolous poison which produces small pox, and no other disease. The effect of the pretended marshy miasm is complete, nor are intermittents the only diseases which it gives rise to in the inhabitants of marshy districts. Cachexies, marasmus, abdominal obstructions, dyspepsia, chlorosis, dropsy, are developed equally with intermittents. What I have said of the petechial and variolous diseases, applies to all the vegetable, mineral, and animal poisons, such as the bites of serpents, the effects of all of which, are specific and characteristic.

"But how does marsh miasma act? In an instant we must suppose—more promptly even than the most active of poisons, the most deadly of contagions, the hydrophobia. A few hours stay, especially in the night, in marshy districts, are sufficient to generate an intermittent."

Our readers will, we suspect, think that the author has mistaken analogical for parallel cases, in reasoning on what he conceives ought to be the identity of contagion and miasm, on the commonly received opinion. A variety of

emanations from the earth, act, we know, on the lungs or stomach, and system generally—and yet we do not think of referring them to the class of morbid contagious agents.

We must also dissent from the proposition advanced above, that diseases acknowledged as contagious, are all propagated by contact, and not through the air. The small-pox and measles afford a conclusive refutation of this notion.

But while it cannot be denied that a residence, or even a short stay, in marshy districts, gives rise to intermitting fevers, our author thinks he can furnish a different explanation of the fact, in the minor evolution of caloric, under such circumstances. This minor evolution is the proximate cause of the cold fit, and follows from the nature of the air of marshes.

Hydrogen and carbonic acid gases abound in them, and the quantity of oxygen is diminished.* Hence, minor absorption of the last gas, and minor evolution of caloric, from the diminished play of the lungs, from which again results the defective elimination of hydrogen and carbon—substances which, retained, have a noxious and sedative effect. It is not then surprising, that in a short time, even in a single night, persons of a feeble habit, should have the nervous system thrown into that state of debility, and the arterial of inertia, which are all that is necessary for the production of a cold fit of intermittent fever.

It is a fact, now of pretty general notoriety, that persons labouring under hæmoptysis, derive great and not unfrequently permanent benefit, from a residence in a marshy country. Wine, and other stimuli, are tolerated by them in this situation, when in a pure air they would produce a relapse. Such an effect cannot be referred to a miasm, or the baneful action of carbonic acid gas, "but to that grade of debility which the air of marshy grounds, for the reasons already assigned, is apt to induce in the nervous and arterial system, by depriving it especially of one of the stimuli

* The analysis of Volta and others, do not, by any means, prove this last assertion—the diminution of oxygen.

which have the principal part in the generation and support of hæmoptysis."

Though we mistrust this chemical theory of the causes of intermittent fever, we think it our duty to adduce some other authorities and facts, which we are acquainted with, to elucidate and confirm the opinion of Giannini.

Humboldt and Ingenhouz have demonstrated the faculty which some earthy substances have of extracting and absorbing oxygen from the atmosphere. The former philosopher exposed some gray sigillated earth, saturated with carbon, to oxygen, and found, that in the space of eighteen days, six-sevenths of the gas had disappeared, and was united with the argillaceous earth. He thence concludes, that the atmospheric air, in contact with a similar substance, must lose a great deal of its oxygen, whilst the quantity of azote will remain nearly the same. What is well worthy of remark is, that the argillaceous and calcareous earths being rendered dry, produced no change whatever on the atmosphere.

It has been proved, by Morozzi and Roupe, that the carbonic gas, being free, easily and spontaneously unites itself to the two other constituent parts of the atmosphere, without causing in them any alteration, but, when it is saturated with azote, and then joined to the atmospheric air, it deprives the latter of a great part of its oxygen. It is not then surprising that similar phenomena should take place in respect to recent cultivated land, since the carbonic gas is there naturally combined with the azote. It follows, from these experiments, that pure vegetative earth, free from heterogeneous matters, never produces upon the atmosphere the pernicious effects which it causes when in conjunction with other species of earth, or other substances which have a great affinity for oxygen. Of the earths, none possess this quality in so great a degree as the argillaceous, when conveniently circumstanced. When, in the places in which it abounds, argillaceous earth is preserved in a state of continual humidity, it causes a considerable subtraction of oxygen; and that the air may acquire that deleterious property,

it is necessary, as Prosper Alpinus, Pringle, Cullen, and others have remarked, that the marshy humid soil should be in immediate contact with the atmosphere; for as long as it is found covered with a certain quantity of water it produces no disease whatever.

To these we may add, that the eudiometrical experiments of Humboldt, in his meteorological tables, show, that in rainy weather the atmosphere contains much less oxygen than in clear calm weather. Now it is notorious, that the principal rains generally fall in spring and autumn, and that it is also in those seasons that intermittent fevers most prevail. As then oxygen gas is that constituent part of the air which stimulates and sustains the animal and vital powers, it follows, that its scarcity in marshy countries must debilitate the vital economy, and produce diseases which directly depend upon such debility.

Such is the hypothesis of Van Aenvank, former professor of chemistry at Louvaine, in a dissertation written in Flemish, entitled " A treatise concerning the Influence of Cultivated Land on the Atmosphere, considered as the principal cause of Intermittent Fevers;" an abstract of which is given in the work of Van Rotterdam, " On Blood-letting in Fevers." It is from this last work that we have borrowed.

In corroboration of the opinion of Van Aenvank, we may mention the observations of Chisholm, in his late work on the Medical Topography of the West India Islands, from which it appears that the soil of Antigua is very argillaceous, and that the moisture, and, consequently, the comparatively great cold, produces fevers similar to those having their origin in miasmata; though in the neighbourhood of St. John's, where the above effects are produced, there are no marshes.

Van Rotterdam, in the work just cited, thinks that the two opinions—the one taught by Cullen, and that presented to our readers—may be reconciled.

" It is by no means contradictory that the putrid miasms, besides their direct sedative action on the cerebral system, may yet have the property of decomposing the atmospheric

air, and of breaking the connexion of its two constituent parts; for it may very readily happen, that these putrid miasms, being of an azotic nature, may attract oxygen, absorb part, and consequently deprive the atmospheric air of it, which, in this state of degeneration, can no longer produce the salutary effects of a wholesome respiration."—p. 60.

To return to the work under review.—The author concludes the second chapter, by expressing his dissent from the opinion of Brown and his followers, that the longer the intermission, the milder is the fever. Whereas, from the premises here laid down, we should say that the longer the intermission, the greater the debility, and the more difficult of removal. Tertians are then easier cured than quartans; and quotidians, where there is a regular intermission, still more so. This, we believe, is conformable to general experience, though many writers, dwelling more on speculation than their own practice, speak of tertians as the easiest of cure. Such is the opinion of Fordyce, who, it must be avowed, generally expresses himself with great caution.—"It is also to be observed," says he, "that a regular tertian goes through its course with greater regularity than an intermittent of any other type, and with less danger to the patient: and it is also more readily carried off by remedies. Quotidians are the next in these points to tertians. A regular quartan is apt to be attended with many more accidents; but not so apt to be irregular as fevers observing any other type, (excepting a quotidian or tertian)."*

We are not sure but what this opinion conflicts with the fact advanced by the same writer, a page or two preceding, that a quotidian has a shorter course, if left to itself, than a tertian. If the former be perfectly regular, it leaves the patient in *about ten weeks;* whereas a regular tertian, going through its natural course, seldom leaves the patient in *less than fourteen weeks*. The quartan is apt to run on for five months, before there are any signs of abatement; and it is

* Fourth Dissertation on Fever.

often *six*, *seven*, or *eight months* before the disease entirely ceases.

Giannini calls our attention to the circumstance of the tertian and quartan being almost universally ushered in by a chill, while quotidians often appear without any—showing that the debility which is measured by the cold fit has not been accumulated so much in the latter as the two former.

The third chapter of the work before us is principally taken up with an explanation of that compound state of the system, which the author believes to exist in the hot stage of an intermittent fever, and to which he gives the name of *Neurosthenia*, consisting of debility and increased excitement. He begins the verification of this state by a notice of some of the physiological principles of Haller and Metzger; the former of whom had spoken of the antagonization of the muscles, by which the relaxation of some was the sole cause of the contraction of others. The latter extended the doctrine still farther, by applying it to the functions of the principal organs in a state of health, as in the motions of the heart, between the auricles and ventricles, and between the left ventricle and the arteries; in the stomach, between the cavity and the pylorus; between the small intestines and the valve of the colon; and the large intestines and the sphincter ani. There is antagonization between the body of the bladder and the neck, at its termination in the urethra. The same takes place in the gall bladder, and in the uterus, between the body and neck. Respiration is a function of a mixed kind; but the elevation of the ribs and contraction of the diaphragm producing a dilatation of the chest is followed by the contraction of the abdominal muscles, the descent of the ribs, and the extension of the diaphragm; hence, between these two sets of muscles there is constant antagonization.

For all practical purposes in therapeutics and pathology, our author considers the division of the body into the three systems of nervous, arterial, and muscular, quite sufficient, and regards the subject of the lymphatic system, and the membranes, " introduced recently into French medicine, and

also any other organ or part of the human frame, as of no practical utility." This language ought to be an important lesson to those who, either slow of comprehension, or rooted to old opinions, obstinately withhold their attention, and assent to any and every new doctrine. Giannini lived long enough to see the physiology of Bichat triumph, and the pathological disquisitions founded on it, particularly those into the morbid alterations of the membranes, nearly supersede all others, and be viewed at this day as subjects of paramount importance in medicine.

"Where the force of the three systems, the grade, the intensity of their action are uniform, there is that equilibrium which constitutes health. Where these reciprocal forces are unequal, there is a corresponding disturbance which constitutes disease."

We do not conceive it necessary or instructive to follow our author through the reasonings in this chapter, which are rather abstractive. He confutes, however, very satisfactorily, that part of the Brunonian doctrine, which, insisting on the indivisibility of sensation, denied the complication of any one disease.

"If an intermittent fever, more especially a pernicious one, be treated by blood-letting, the symptoms increase in strength and gravity. Above all, the *neurosthenic* orgasm of the hot fit is increased. It is evident that this depends entirely on the primitive preponderance of the action of the arterial system over the nervous. And it is from the increased debility of the nervous system that the movements of the arterial become continually more preponderant, in spite of the bleeding, which should have diminished it. Let the venesection be carried beyond bounds, or repeated, in the same pernicious intermittent, the arterial reaction of the hot fit does not take place—the patient dies in the chills. A proof this, that the increased debility of the nervous system is alone sufficient to develop an increased action of the arterial to a certain point, beyond which, the nerves affected with mortal debility, are themselves destitute of that attribute which renders them sensible to the action of stimuli."

"The loss then of the natural equilibrium in the respective power of the three systems, nervous, arterial, and muscular, is the cause of the neurosthenic complication."

"Every degree of debility may give rise to neurosthenic. It may be developed at ten degrees of debility, constituting a slight disease, and at twenty, forming a very alarming one. We shall

see that these two neurosthenias, only differing in their grade, are productive of different phenomena for their cure. We shall see also, that their cure may be accomplished by a double method—since, where the neurosthenia has a slight foundation of debility, it may be cured; or, to speak more correctly, transformed, by increasing even the debility, by curative means—but where it is great, the same method might prove fatal."

"In neurosthenia, though the nervous system be always affected, the arterial and muscular systems are not always so.

"The muscular system alone may be implicated. The arterial system alone may be affected. A part only of the muscular system may be affected:—and a part only of the arterial similarly situated.

"Both the systems, muscular and arterial, may be implicated contemporaneously.

"The arterial is generally brought into consent, when the muscular is entirely affected in some sensible part of it.

"The arterial is brought into consent in the beginning of a muscular neurosthenia, and ceases to participate in it when the latter becomes chronic.

"The constant symptom of arterial complication is increase of caloric.

"In the arterial complication, the increased heat is the cause of the other symptoms.

"Muscular complication may exist when the natural heat continues.

"When, in neurosthenia, the whole arterial system is affected, we have the neurosthenia diathesis, or neurosthenia with fever. In other cases the neurosthenia is without diathesis; that is, it is local."

This exposition of the compound nature of the morbid excitement, which prevails in intermittent and other fevers, will, we apprehend, lead to a more successful practice than any of the commonly received theories which make the cause of the former to consist in debility, or collapse of the brain, or derangements of the primæ viæ. We have now double and apparently opposite indications to follow. Stimulate and remove the debility, and diminish the excessive excitement which is complicated with it. The class of stimulating remedies easily answers the first intention. But it is not every debilitating remedy which accomplishes the second. Blood-letting will promptly diminish the excitement, but at the same time abstracts a stimulus, which in

neurosthenia had been previously deficient. For similar reasons purgatives and other directly debilitating remedies are contra-indicated, as fasting, &c. To the question—what then will diminish the excessive excitement peculiar to neurosthenia, our author replies, cold immersion, the object of which is to facilitate the administration of corroborants.

We can, on the principles here laid down, account for the partial relief afforded by two very opposite kinds of remedies, viz. venesection and opium. The former, by reducing the excitement of the arteries and muscles, brings them on a par of debility with the nerves, and for a time relieves the neurosthenia. A recurrence of the paroxysm, if met in the same way, is followed by the same effects, until all the symptoms become nervous, the debility is extreme, and the convalescence tedious. Opium and other diffusible stimuli, on the other hand, excite the nervous system, and produce an equilibrium between it and the arterial and muscular, and at the risk of producing continued fever, or visceral engorgement may remove the primary disease.

These results are calculated to enforce most strongly the necessity of cautious reasoning in medicine, and the necessity of setting out on correct principles. We must ever be guided by some system, and that assuredly is most entitled to our confidence which embodies the greatest number of facts, and accounts for the greatest variety of phenomena, and even corrects in a measure the fallacies of experience.

The belief in sthenic intermittents following from the doctrine of Brown, rather than directly acknowledged by him, implies even on his own principles a manifest contradiction. For, how can we class, under the head of sthenic disease, a fever which he considered as exclusively asthenic. Though inclined at one time to the first opinion, our author in the work before us very frankly recants and acknowledges his error.

The difficulty frequently experienced of curing intermittent fevers by bark, and other tonics, does not proceed from the sthenic nature of the disease, but from the indistinctness of the paroxysms, or still more from there being a

slight fever during the intermission, whereby the system is kept nearly in the same state as in the hot fit. A perfect apyrexia once procured, and this is better accomplished by the cold immersion than by any other means, the bark and other tonics will then complete the cure. So far practice. Theory excludes the notion of sthenic intermittents in the general chain of causes and effects. The cold, hot and sweating stages, and the intermission dependent on each other, in the order mentioned, present nothing peculiar or different from the same succession in the avowedly asthenic intermittents. Our author cites cases occurring in his own practice, and analyzes the opinion of Brown, Frank, and others, with the effect of confirming the negative side of the question, which he supports.

As our object has been to give the views of others, and not a system of our own, discrepancies of opinion between the author noticed in our last, and the one now under review, must not excite surprise.

Having already exceeded our proposed limits, we must close the present article with expressing our intention of continuing the review of our author's opinions and practice in other febrile diseases, in a future number of this Journal.

Art. XI. *Acta Regiæ Societatis Medicæ Harniensis*, Vol. VI. *Harniæ*, 1821.

WE received some time ago, through the hands of Mr. Pedersen, the Danish minister to the United States, who has long been distinguished among us by liberal views, and elegant accomplishments, the patron of our own, and the procurer for our use of the works of continental Europe, the above interesting volume of the transactions of a learned society of Copenhagen.*

* We are glad to find, that Mr. Pedersen has been elected a member of the American Philosophical Society, the highest literary honour which it was in our power to bestow on him.—Editor.

The duty of a reviewer of medical books, is essentially different from that of the critic of the productions of general literature. The latter seldom confines himself to the work before him, snd sometimes may be said to quit it altogether. He commonly entertains us with an original disquisition upon the subject of consideration—and if he condescends to notice the author at all, it is in the authoritative tones of a judge, from whom there is no appeal. In our science, on the contrary, the humble analyst must generally confine himself to the lesson before him—in the modern style of book-making, often a very tedious one, and endeavour to convey, as far as he can, the greatest amount of knowledge in the fewest words. A caterer to those whose occupations and situations prevent the use of the more copious store of medical literature, his province is to read, in order to reject what it is not desirable to read—and to toil like a poineer of his profession, amid dust and rubbish, that the path may be straight and smooth for those who are to come after him.

But, though his pretensions are thus moderate, and of the laborious class, it would be a dereliction of his duty not to speak, in passing over the contents of a work, of the merit which the work possesses. Much time is thus saved to the reader, and there are many to whom the mere fact of the expression of a collateral opinion affords a satisfaction. Thus far we felt a desire to explain our views on the expression of judgment in treating of medical works, without, however, any particular reference to that which we are now considering.

It may be stated, generally, that the contents of this volume of transactions, are valuable, and very honourable to the industry and research of the members of the society which has produced it. We cannot agree with all the practice which it describes, and as exceptions, we may cite armenian bole and hæmatite in hemorrhage—warm regimen in measles, musk in exceedingly small doses, &c. Yet it shows a disposition diligently to investigate nature, and to embrace the discoveries of others, the only safe-guards for

the improvement of the medical art. Most of the essays betray close study of the writers of Great Britain—and in a less degree, of those of France. With these preliminary remarks, we proceed to give a more minute account of the contents of the work.

Art. I. is an account of a successful operation for popliteal aneurism, by H. Eastner. He operated immediately above the point where the pulsations of the crural artery disappeared from the touch when examined from the groin downwards. The vessel was found and exposed in this place immediately upon the first incision, an advantage not without its value. Two ligatures were employed, but in place of dividing the artery between them, the limb was simply laid so as to relax the parts to the utmost, and this was thought to have answered equally well. After three months, the tumour not having diminished in size, and yet being without pulsation, was opened and evacuated. Considerable oozing of blood from the whole surface followed, which was stopped with lint, and in seven weeks the wound healed.

Art. II. is an account of eight cases of poisoning by sulphuric and nitric acids, by C. M. Lunding. Death, when it did occur, was after long protracted tortures. The poisons seemed to have a peculiar tendency to injure the pyloric end of the stomach. No exanthema, nor vomitings of portions of the internal membrane, were observed.

Art. III. is an account of a case of pulmonary phthisis, with a dissection.

Art. IV. of an abscess in the scrotum mistaken for a hernia.

Art. V. of three cases of intussusception.

Art. VI. describes a case of insidious inflammation of the mesentery, similar, as the writer, J. C. Wendt, informs us, to several others which had occurred to him. The patient, a boy of five years old, was left in a weak state from a convulsive cough—when suddenly he complained of pain in his limbs. Next day he was seized with vomiting, great redness of the eyes and stupid expression of countenance.

The pulse and tongue natural. The child languid and occasionally delirious. The abdomen was not swelled nor *painful* on pressure—but the boy complained of pain in his loins. He sunk into a typhoid state, and expired on the fifth day. On dissection, the portion of the mesentery connected with the whole length of the jejunum was inflamed, extending more widely along the back, but not affecting the intestine. Three dissections are mentioned, in which ulcerated openings were discovered in the mesentery, discharging pus copiously. The author believes these cases not to be uncommon.

Art. VII. is a case of caries of the face, which recovered under unfavourable circumstances.

Art. VIII. An account of ten cases of puerperal fever, by O. Bang.

Art. IX. A case of fatal and long continued hemorrhage from the anus, by W. Klingberg.

Art. X. A case of ileus, in which cold water, externally applied by cloths, was of remarkable service.

Art. XI. An account of two epidemics of the measles, in 1791 and 1820, by the father and son, Bang.

Art. XII. Of some experiments made with prussic acid, on horses, dogs, sheep and goats—principally with a view to ascertain the relative effect upon these animals.

Art. XIII. is on a syphiloid disease prevalent in the country called the Marsk, at Bordesholm in Holsatia, and in Finland, and bearing the title of "Morbus Ditmarsiensis," from the provincial name. We shall quote some of his remarks.

"Morbus est contagiosus, et rarissime certe, si unquam, hac ratione non gignitur. Modus quo contagium est receptum, et quando, haud semper certe eruendus—est tamen coitus nec solus nec etiam forte frequentior habendus. Neque ætati, neque sexui, neque cuidam corporis habitui, nec parcet nec favet morbus—medie tamen vitre maxime communis. Frequentissime apud ruricolas eosque pauperes observatur, rarius apud urbanos opulentosque. Signa morbi maxime cum iis in vera lue cognitis conveniunt, hæcque

sunt—ulcera faucium, dolores osteocopi, corona venerea, aliaque exanthemata, tophi, gummata, condylomata."

A peculiar small tubercle is then described, which is converted into slow ulcerations—to be treated with aqua phagedenica, and the internal use of red precipitate.

"Remedium morbo valentissimum, me judice, offert *mercurius*, nec ullo fere modo curatio ejus ab ea luí adpropriata differt. Ossium affectus rarius tollentur. Morbus ubi, quod raro quamvis interdum accidit, mercurio resistat, dyscrasiam quandam humorum, natam maxime e scorbuto aut scrofulis, statuendam existimo." The whole of the papers of this volume are written in the Latin language.

The disease, we are told, bears a strong resemblance to sibbens, and to that called "radesyge," in Norway. Our readers may recollect a venereal affection, mentioned among the medical intelligence, at the end of one of our previous numbers, which was supposed to be communicated by contact, without impure connexion.

Art. XIV. is a description with a plate, by L. L. Jacobson, of an aqueous humour, discovered by him, between the retina and tunica choroidea, and of certain diseased conditions of it. These are, 1st, a distension and alteration of form in the globe of the eye—being what Scarpa calls *staphyloma posticum*, which effect is produced by an increased quantity of the fluid. 2d, Lapideous concretions, adapted to the form of the cavity, and containing the origin of the retina, in a narrow canal extending through it. These he considers to have been mistaken for ossifications of the tunica or of the vitreous humour.

Art. XV, and last, is a dissection, in which a calculus was found included in a cavity appertaining to the bladder, beneath the integuments of the hypogastric region.

Art. XII. *Lessons in Practical Anatomy, for the use of Dissectors.* By W. E. Horner, M. D., Adjunct Professor of Anatomy in the University of Pennsylvania.

It is with much pleasure we lay before our readers a brief notice of the work lately published by Dr. Horner, which is much to be valued, as coming from one who has so long been practically conversant with the subject on which he writes.

The real and factitious difficulties impeding the advances of the student of anatomy, are very numerous, although the science of the human frame is not so inaccessible, as is generally supposed. The want of opportunity, the disagreeableness of dissecting, and the absence of experienced assistance in cases where all other difficulties are removed, may be ranked among the real or necessary hindrances. The factitious or unnecessary obstacles can scarcely be recounted, though improper modes of study, the adoption of unfounded prejudices, erroneous views of the importance of anatomy relative to the practice of medicine, no less than as to its peculiar nature, and reliance on defective books for direction in the study of practical anatomy, may be esteemed among such as are most injurious to the learner and prejudicial to the interests of the science.

The object of this work, is to remove as much as possible the difficulties attendant on the study of practical anatomy, and to facilitate the acquisition of anatomical knowledge: to give the student a strongly and faithfully made sketch, the detail of which is to be supplied by his own application. Hence, the descriptions are necessarily divested of extreme minuteness, or an affectation of subtilized precision: the style is plain, unostentatious and didactic, intended for the instruction of beginners, not designed to display the author before such as have advanced beyond the middle term of this extensive and admirable science.

The method adopted by our author, that of considering the whole of the body in three parts, he has chosen, because, from long observation, he conceives it to be the best. Whatever opinion we may form as to the abstract correctness of such a division, we are willing to withhold any objections which are not founded on equal experiment, seeing that *the experience* of the author has convinced him of its usefulness.

In reality, it is of more consequence *how*, than *where* we begin, in anatomy. The individual who has studied in theory or altogether from books and plates, has a host of miserably incorrect preconceptions to relinquish and a multitude of erroneous descriptions to forget. He must not only forego his own notions on the subject, but must struggle to shake off that influence which the doctrines and dogmata of authors have imperceptibly acquired over his mind. It is much better, therefore, to begin by using such a work as that under consideration, and let the reading be followed by immediate practice. This at once tests the correctness of the author and makes an indelible impression on the mind of the learner, who is the more incited to advance, as he becomes convinced of the capability of his guide.

Dr. Horner enjoys the rare happiness of having made a discovery, in a science which was thought to be too well explored to allow an investigator the slightest hope of such a reward. He has discovered a muscle in the inner angle of the orbit, which till then, had escaped the research of so many anatomists. This muscle leads us to a clearer understanding of the beautiful apparatus of vision, and adds another proof of the omniscience of the great first cause who has adapted the wonderful contexture of our system, part to part, and each to the whole.

In justice to our author, we give his own description of this muscle, only delaying to remark that by many dissections we have convinced ourselves of the correctness of his relation.

" At the internal canthus of the orbit, is a small muscle belonging to the internal commissure of the eye-lids which has

not been observed before, or is omitted in the descriptions of the part. That it does not belong to either of the above, [obliquus superior, et inferior] or to the orbicularis palpebrarum, a reference to very minute accounts of them, given by the most eminent anatomists, will prove.

"This muscle is about three lines broad, and six lines long—arising from the posterior flat surface of the os unguis, near its junction with the os æthmoides, and passes forwards, and outwards, lying on the posterior face of the lachrymal ducts. As it approaches the commissure of the lids, it splits into two parts, nearly equal, each of which is appropriated to a duct, and inserted along its course, almost to the punctum lachrymale.

"To get a distinct view of it, the eye-lids must be separated from the eye, and turned over the nose, leaving the tendinous attachment of the orbicularis and ciliaris muscles. The valvula semilunaris is brought into sight by this process, which must be dissected away, and also the fat and cellular membrane underneath it. The muscle is now seen, and by passing bristles through the lachrymal ducts, its connexion with them is rendered evident, at the same time we get a good idea of its size, origin, and insertion. While making this inspection, by turning the muscle somewhat aside, we shall be rendered sensible of another fact of some importance—that the attachment of the inner commissure of the eye-lids to the internal canthus of the orbit is imperfectly described, even by anatomists of much minuteness in their accounts. It is attributed exclusively to the tendon of the orbicularis muscle, so much so that in the operation for fistula lachrymalis, we are enjoined not to cut through the tendon, least a puckering of the eye-lids be produced by their line of extension being destroyed. The fact, on the contrary, is, that a ligamentous matter behind this tendon, passes between the internal ends of the eye-lids and the posterior flat surface of the os unguis—so that, admitting the tendon of the orbicularis to be cut through, this ligament, assisted by the little muscle described, would prevent the dreaded deformity. The internal extremity of this posterior ligament, is at least half an inch from the insertion of the orbicularis tendon into the nasal process, and it brings into a curve commonly seen at their junction. The lachrymal ducts are involved in this posterior ligament, passing along it into the sac, instead of going along the edges of the commissure, as commonly described just under the skin. The muscle attempted to be described, must influence considerably the position of the puncta lachrymalia, by drawing them towards the ball of the eye, and keeping them in close contact with it: it is, therefore, a very efficient means for regulating so far, the lachrymal passages, and for securing the course of the tears. I am indebted to Dr. Physick for a further suggestion in

regard to its other uses, which appears highly probable. In cases of extreme emaciation, it is well known that the adipose matter around the ball of the eye, is more or less absorbed, causing it to sink deeper into the orbit, and consequently to retire somewhat from the lids. The effect of the muscle is to draw the lids backwards, and to keep them applied on the ball. Again, in the elevation of the upper lid, or rather the drawing of it within the orbit by the levator palpebræ, the tendency of the margin of the lid is to leave the ball; the upper part of the little muscle obviates this tendency. As such appears to be the actions of the part, I must, therefore, coincide with him in calling it *tensor tarsi*, a name expressive of its functions." P. 116, &c.

To obtain a view of this muscle in its natural relations, or without reversing it, we may introduce a bristle into each punctum, and cut the orbicularis oculi across, about the sixteenth of an inch from the puncta, and turn this part towards the external angle. The small flap of the orbicularis may then be raised and turned over on its tendon, which may be divided so as to allow us to raise sufficiently, without entirely removing it. By pressing on the ends of the bristles, so as to raise and tighten the puncta, we see at once the course of the muscle, and may, with great facility, display its fibres as they stretch out on the puncta, as they come off from the common origin.

Dr. Horner has properly added to the anatomy of the brain, the method of investigation adopted by Gall and Spurzheim. Although their anatomical discoveries have, as yet, no connexion with their physiological doctrines, their method may be advantageously resorted to by the student, and may eventually lead to a more satisfactory acquaintance with the hitherto inexplicable nervous system.

It is not from any disposition to slight the work before us, that we do not offer a regular analysis of its contents, but because a system of anatomy *must be* similar, in the main, to those before published. The reputation and experience of the author will insure it a full share of public attention, and time clearly demonstrate its merit. For ourselves, we have read it with profit and pleasure, recognizing throughout, the fidelity of the outline and the truth of the perspective.

To say that there are some imperfections and inaccuracies in this work, is what may be said of any. We do not think a particular indication of these necessary, as in a second edition they will no doubt be corrected. We would suggest to the author, that an alteration of such technicals as are now partly given in Latin, and partly in English, to either language, would be productive of benefit. In one or two instances, technicals are abridged, as if for convenience, either of the writer or printer. This is inadmissible in an elementary work, especially when the words so abridged are in a foreign language.

The work is with great propriety dedicated to Professor Physick, whose reputation, now elevated above accident or change, has become a national property, as his benevolence and professional skill have long been national benefits.

"Tantam doctrinam, tantamque omnium quæ ad usum medicum pertinent rerum scientiam, insigni, pietate, modestia, et benignitate, moribusque honestissimis et simplicissimis ornavit; miraque felicitate gavisus *est*, suam de ne, quavis scientiam informan concinnam perspicuam et elegantem redigendi.*

We should feel wanting in candour as well as enthusiasm, in all that relates to professional excellence, if we neglected any opportunity to express our veneration for this truly great man, whose approbation is sufficient to stamp with value any work in his peculiar department of science.

* I. Gregorius.

BIOGRAPHY.

ART. XIII. *A Tribute to the Memory of the late Dr. Benjamin Rush.* By DAVID HOSACK, M. D. Professor of the Theory and Practice of Physic and Clinical Medicine in the University of New York. (Delivered before the Class.)

DOCTOR BENJAMIN RUSH was born on the 24th of December, 1745, on his father's estate, about twelve miles from the city of Philadelphia. His ancestors followed William Penn from England to Pennsylvania, in the year 1683. They chiefly belonged to the society of Friends, and were all, as well as his parents, distinguished for the industry, the virtue, and the piety, characteristic of their sect. His father John Rush, died while his son Benjamin was yet young, but left him to the care of an excellent and pious mother, who took an active interest in his education and welfare. In a letter addressed to the Honourable John Adams, late President of the United States, a short time before his death, and which was written upon his return from a visit to the tomb of his ancestors, he thus expresses the obligation he felt for the early impressions of piety he had received from his parents: "I have acquired and received nothing from the world which I prize so highly as the religious principles I inherited from them—and I possess nothing that I value so much as the innocence and purity of their characters."*

But this was not the only source of that virtue and religion for which he was so eminently distinguished. His mother, as if influenced by a presentiment of the future des-

* The letter here referred to was originally addressed, by Dr. Rush, to the Hon. John Adams, Esq. late President of the United States: from a copy of the same, sent to the author by Dr. Rush, several of the preceding interesting particulars have been taken.

tinies of her son, resolved to give him the advantages of the best education, which our country then afforded—and for this purpose he was sent, at the early age of eight or nine years, to the West Nottingham Grammar School, and placed under the care of his maternal uncle, the Rev. Doctor Samuel Finley, afterwards President of the College in Princeton, New Jersey. At that school young Rush remained five years—and there acquired a knowledge of the Greek and Latin languages, and the other branches preparatory to a collegiate course of study. Under the tuition and guidance of Doctor Finley, he was not merely instructed in classical literature;—he also acquired what was of no less importance, and which characterised him through life—a habit of study and observation, a reverence for the Christian religion, and the habitual performance of the duties it inculcates. At the age of fourteen he was removed to the College of Princeton, then under the superintendence of President Davies, one of the most eloquent preachers and learned divines our country has produced. At college our pupil performed his duties with his usual attention and success: he became distinguished for his talents, his uncommon progress in science, and especially for his eloquence in public speaking. He was no less admired for the brilliancy of his genius, than beloved for the goodness of his heart. "Even in the hey-day of youth" he was distinguished for his integrity and decorum: "he needed not the sting of guilt to make him virtuous, nor the regret of folly to make him wise."*

He received the degree of bachelor of arts in the autumn of 1760, at the early age of fifteen. The next succeeding six years of his life were devoted to the study of medicine, under the direction of Doctor John Redman, at that time an eminent practitioner in the city of Philadelphia. Upon commencing the study of medicine, the writings of Hippocrates were among the very first works which attracted his attention. From them he probably derived that talent of

* Life of Fisher Ames.

investigation, that spirit of inquiry, and those extensive views of the nature and causes of disease, which give value to his writings, and have added important benefits to the science of medicine. The same mode of acquiring knowledge which was recommended by Mr. Locke, and the very manner of his common place book, was also early adopted by Doctor Rush, and was daily continued to the last of his life. To his records, made in 1762, we are at this day indebted for many important facts illustrative of the yellow fever, which prevailed in, and desolated the city of Philadelphia, in that memorable year. Even in reading, it was his practice to mark any important fact or any peculiar expression, remarkable either for its strength or its elegance. Like Gibbon, "he investigated with his pen always in his hand." Having with great fidelity completed his course of medical studies under Dr. Redman, he embarked for Europe, and passed two years at the University of Edinburgh, attending the lectures of those celebrated professors, Dr. Monro, Dr. Gregory, Dr. Cullen, and Dr. Black: and in the spring of 1768, after defending an inaugural dissertation "de coctione ciborum in ventriculo," he received the degree of doctor of medicine. In that exercise, which was written with classical purity and elegance, it was the object of Dr. Rush to illustrate, by experiment, an opinion that had been expressed by Dr. Cullen, that the aliment, in a few hours after being received into the stomach, undergoes the acetous fermentation. This fact he established by three different experiments, made upon himself—experiments, which a mind less ardent in the pursuit of truth would readily have declined.

From Edinburgh, Dr. Rush proceeded to London, where, in attendance upon the hospitals of that city, the lectures of its celebrated teachers and the society of the learned, he made many accessions to the stock of knowledge which he had already acquired.

In the spring of 1769, after visiting Paris, he returned to his native country, and immediately commenced the

practice of physic in the city of Philadelphia, in which he soon became highly distinguished.

Few men have entered the profession in any age or country with more numerous qualifications as a physician than those possessed by Dr. Rush. His gentleness of manner, his sympathy with the distressed, his kindness to the poor, his faithful attention to the sick, his professional acquirements and his varied and extensive erudition, all united in procuring for him the esteem, the respect and the confidence of his fellow citizens, and thereby introducing him to an extensive and lucrative practice.

It is observed, as an evidence of the diligence and fidelity with which Dr. Rush devoted himself to his medical studies, during the six years he had been the pupil of Dr. Redman, that he absented himself from his business but two days in the whole of that period of time. I believe it may also be said, that from the time he commenced the practice of medicine to the termination of his long and valuable life, except when confined by sickness, or occupied by business of a public nature, he never absented himself from the city of Philadelphia, nor omitted the performance of his professional duties a single day. It is also stated, that during the thirty years of his attendance as a physician to the Pennsylvania hospital, such was his punctuality, his love of order, and his sense of duty, that he not only made his daily visit to that institution, but was never absent ten minutes after the appointed hour of prescribing.

In a few months after his establishment in Philadelphia he was elected to the professorship of chemistry in the medical school, which had then been recently established by the laudable exertions of Dr. Shippen, Dr. Kuhn, Dr. Morgan and Dr. Bond. For this station his talents and education peculiarly qualified him. As in the case of Boerhaave, such too had been the attention bestowed by Dr. Rush upon every branch of medicine, that he was equally prepared to fill any department in which his services might be required.

The professorships of anatomy, the theory and practice of physic, clinical medicine, and the materia medica, being

already occupied, he was placed in the chair of chemistry, which he filled in such manner as immediately to attract the attention of all who heard him, not only to the branch he taught, but to the learning, the abilities, and eloquence, of the teacher.

In the year 1789 he was elected the successor of Dr. Morgan to the chair of the theory and practice of physic. In 1791, upon an union being effected between the College of Philadelphia and the University of Pennsylvania, he was appointed to the professorship of the institutes of medicine and clinical practice—and in 1805, upon the resignation of the learned and venerable Dr. Kuhn, he was chosen to the united professorships of the theory and practice of physic and of clinical medicine, which he held the remainder of his life. To the success with which these several branches of medicine were taught by Dr. Rush, the popularity of his lectures, the yearly increase of his pupils, the unexampled growth of the medical school of Philadelphia, and the consequent diffusion of medical learning, bear ample testimony—for, with all due respect to the distinguished talents with which the other professorships of that University have hitherto been, and still continue to be filled, it will be admitted, that to the learning, the abilities, and the eloquence of Dr. Rush, it owes much of that celebrity and elevation to which it has attained. What Boerhaave was to the medical school of Leyden, or Dr. Cullen to that of Edinburgh, Dr. Rush was to the University of Pennsylvania.

But he did not confine his attention and pursuits either to the practice of medicine, or to the duties of his professorship: his ardent mind did not permit him to be an inactive spectator of those important public events which occurred in the early period of his life.

The American revolution—the independence of his country—the establishment of a new constitution of government for the United States, and the melioration of the constitution of his own state, all successively interested his feelings, and induced him to take an active concern in the scenes that

were passing. He held a seat in the celebrated Congress of 1776, as a representative of the state of Pennsylvania, and subscribed the Declaration of American Independence. In 1777, he was appointed physician general of the military hospital for the middle department—and in the year 1787, he was chosen a member of the state convention for the adoption of the federal constitution.

These great events being accomplished, Dr. Rush resolved gradually to retire from political life, and to dedicate the remainder of his days to the practice of his profession, the performance of his collegiate duties, and the publication of those doctrines and principles in medicine, which he considered calculated to advance the interests of his favourite science, or to diminish the evils of human life. In a letter which I received from him as early as the year 1794, he expresses this determination, adding, " I have lately become a mere spectator of all public events:" and in a conversation on this subject, within the two last years of his life, he expressed to me the high gratification which he enjoyed in his medical studies and pursuits, as well as his regret that he had not at a much earlier period withdrawn his attention from all other subjects and bestowed it exclusively upon his profession.

Young gentlemen, let this declaration of that venerable character, who, like Hippocrates of old, well knew the extent of his art, and the comparative shortness of human life, impress your minds with the duties before you. Let it teach you, too, the value of time, that it may not be occupied in those pursuits which are unconnected with science or your profession—and, especially, that it be not wasted in idle and unprofitable amusements: for, of the physician, it is not enough to say,

" That here he liv'd, or here expired."—Pope.

Such was the attachment of Dr. Rush to his profession, that speaking of his approaching dissolution, he remarks, " when that time shall come, I shall relinquish many attractions to life, and among them a pleasure which to me has no equal in human pursuits—I mean that which I derive from

studying, teaching, and practising medicine." But he loved it as a science: principles in medicine were the great objects of all his inquiries. He has well observed, that medicine without principles is an humble art, and a degrading occupation: but directed by principles, the only sure guide to a safe and successful practice—it imparts the utmost elevation to the intellectual and moral character of man.

But the high professional character and attainments of Dr. Rush did not alone display themselves in his skill as a physician, or his abilities as a teacher. He was equally distinguished as a writer and an author.

The limits of this sketch will not allow even a recital of the numerous subjects of his medical publications—much less does it afford an opportunity to review the opinions they contain. It may, however, be remarked, that his bold and exuberant imagination led him to adopt some theories not wholly founded in truth. But they show the march of a mind grand in conception, sublime in result—and if there be error, it is error of a systematic kind.—Si non sunt vera saltem sunt inter se apta. It will be admitted by all that the numerous facts and principles which his writings contain, the doctrines they inculcate relative to the nature and causes of disease, and the improvements they have introduced into the practice of medicine, recommend them to general attention—while the perspicuity and elegance of the style in which they are written, give them additional claim to notice as among the finest models of composition. The same remarks are equally applicable to the epistolary style of Dr. Rush and that of his conversation—in both of which he eminently excelled. Of his conversation it may be said, that such were the riches of his mind—such was the active employment of all its faculties—so constant was his habit of giving expression to his thoughts in an extensive correspondence—in the preparation of his public discourses, and in his daily intercourse with the world, that few persons ever left his society without receiving instruction, and expressing their astonishment at the perpetual "stream of eloquence" in which his thoughts were communicated.

The following are among the most important works of Dr. Rush. Four volumes of Medical Inquiries and Observations—a volume of Essays, Literary, Moral and Philosophical—a volume of Introductory Lectures, and another volume of Medical Inquiries and Observations on the Diseases of the Mind. Many other valuable tracts are to be found in the periodical publications, both of this country and Europe, which our limits will not permit us to enumerate.

It has frequently been the subject of surprise, that amidst the diversified avocations of Dr. Rush, as a practitioner and a teacher of medicine, he found leisure for the composition and the publication of the numerous medical and literary works which have been the production of his pen.

Although he possessed by nature an active and discriminating mind, in which were blended great quickness of perception, and a retentive memory—although he enjoyed the benefit of an excellent preliminary and professional education, it was only by habits of uncommon industry, punctuality in the performance of all his engagements, the strictest temperance and regularity in his mode of life, that he was enabled to accomplish so much in his profession, and to contribute so largely to the medical literature of his country. Like most men who have extended the boundaries of any department of human knowledge—who have contributed to the improvement of any art or science, Dr. Rush was in the habit of early rising, by which he always secured what Gibbon has well denominated "the sacred portion of the day."

The great moralist* justly observes, that "to temperance, every day is bright, and every hour is propitious to diligence." The extreme temperance of Dr. Rush, in like manner, enabled him to keep his mind in continual employment, thereby "setting at defiance the morning mist and the evening damp—the blasts of the east, and the clouds of the south."† He knew not that "lethargy of indolence" which

* Dr. Johnson.

† Boswell, vol i. p. 260.

follows the inordinate gratifications of the table. His ciesto did not consist in indulgence upon the bed or in the armed chair, to recover those powers which had been paralysed or suspended by an excessive meal, or the intemperate use of vinous or spiritous drinks.

Dr. Johnson, during his tour to the Hebrides, when fatigued by his journey, retired to his chamber and wrote his celebrated Latin ode, addressed to Mrs. Thrale.* Dr. Rush, in like manner, after the fatigues of professional duty, refreshed his mind by the perusal of some favourite poet, some work of taste, some volume of travels, biography, or history. These were the pillows on which he sought repose.

But the virtues of his heart, like the faculties of his mind, were also in continued exercise for the benefit of his fellow men. While the numerous humane, charitable, and religious associations, which do honour to the city of Philadelphia, bear testimony to the philanthropy and piety which animated the bosom of their departed benefactor—let it also be remembered, that, as with the good Samaritan, the poor were the objects of his peculiar care—and that in the latter and more prosperous years of his life, one seventh of his income was expended upon the children of affliction and want. Dr. Boerhaave said of the poor, that they were his best patients, because God was their paymaster. Let it be recorded, that, in like manner, the last act of Dr. Rush was an act of charity, and that the last expression which fell from his lips, was an injunction to his son, 'be indulgent to the poor.'

"Vale egregium academiæ decus! tuum nomen mecum semper durabit—et laudes et honores tui in æternum manebunt."†

He died on the 19th of April, 1812, of the peripneumonia typhoides, in the 69th year of his age. His death produced great and universal emotion throughout the United States:

* Boswell.

† These words were addressed by Dr. Rush, upon his taking leave of the University of Edinburgh, to his particular friend and preceptor, Dr. Cullen. See Inaug. Diss. De Coctione Ciborum. Edin. 1769.

man was better known or more revered. His name will ever keep "the name of this country respectable in every quarter of the globe"*—and its lustre will remain unimpaired by the corroding influence of time, "when the eye of malignity is sealed up in darkness, and the tongue of calumny is fettered with the irons of death."†

Art. XIV. *Obituary.* Dr. J. O'B. Lawrance.

THE character of this singular and admirable man, needs not, nor shall receive common or exaggerated praise. A plain and faithful account of the truth will do more justice to the dead.

Dr. John Valentine O'Brien Lawrance was born in the city of New Orleans, in the year 1791. His father had emigrated from New Jersey: his mother was of Irish extraction. His early education was at the schools then afforded by his native city—after attending which till his fifteenth year, he entered at Lower Dublin Academy, near Philadelphia. When his stay in this seminary, which extended to upwards of three years, was expired, he returned to New Orleans, to commence the study of medicine, under his step-father, Dr. Flood. Here he possessed practical opportunities not always enjoyed by the pupils of physicians in large cities—having frequently charge, in the violent and acute diseases of that climate, of a part of his father's patients.

In December he came again to Philadelphia, and became one of the pupils of Dr. Physick, then professor of surgery, whose private friendship he had the hônour of enjoying during the remainder of his life. After distinguishing himself among the class for talents in the acquisition of knowledge, and a remarkable degree of assiduity in any employ-

* Burke. † Dr. Mason.

ment, however laborious or disgusting, which belonged to his profession, he entered the Pennsylvania Hospital in 1814, to fill a temporary vacancy as house physician and surgeon. In this establishment he remained till the ensuing spring, when he graduated, and soon after returned to his native city, to commence the practice of physic, under the paternal auspices of Dr. Flood. Here he immediately obtained a large and lucrative business, and continued till he left that city, the acting physician and surgeon of the New Orleans Hospital, of which his step-father was principal. Dr. Lawrance could not, however, remain long satisfied in this situation. The recollection of the advantages which Philadelphia possessed in every scientific point of view, an early attachment, the friendships which he formed there, and particularly, its great facilities for the prosecution of his favourite pursuit, the study of anatomy, worked upon his mind, until he finally resolved to sacrifice the present possession of a large, profitable and increasing practice, with the best grounds for confidently expecting, at an early period, to reach the summit of professional eminence in the place of his birth, for the object of living where he could to more advantage pursue his inquiries into nature. The increase of knowledge was a tonic to his mind, with which he could not dispense—and every consideration of ambitious or pecuniary advantage was small in the comparison. From the period of his settling in Philadelphia, Dr. Lawrance was obliged to buffet all the difficulties to which those physicians are subjected who settle in large cities, and particularly in our own. His talents were universally, and with pleasure, acknowledged by his numerous that usually termed indefatigable, was obvious to all—his experience and acquirements were great and generally known—his conversation was courted with pleasure and acquaintance—his industry, which was of a kind beyond pride by the first names in science which our city affords, and with some of them he was intimately bound in the relations of private friendship: yet neither talents, nor industry, nor learning, nor experience, nor influential friendship

could supply the place of the opportunities which he had so magnanimously relinquished—and it is believed that, though his prospects were fast brightening towards the close, he continued to struggle with difficulty till the termination of his useful life. An attack of the epidemic fever, which has in such a distressing manner visited the neighbourhood of our city, augmented by a continuance of the unparalleled exertions which he was in the constant habit of making, rapidly hurried him to his end. He was taken ill on the ninth of August, 1823, while visiting in the infected neighbourhood of the Ridge-road, and imprudently continued to labour in the day and curtail his rest at night, till the eleventh, when he was obliged to be conveyed home, in the carriage of a friend, from an operation at the Alms-house. He immediately took to his bed, soon became delirious, rapidly sunk, and in defiance of the best medical attendance, on the nineteenth he expired.

Thus was society deprived of a man, of whom, although it had already began to award him fame, it had never known the value. Assiduous and noiseless in his pursuits, he was, perhaps, the individual whose real merit bore the largest proportion to his pretensions. Always actuated by the love of science and of his species, he was uniformly more ready to labour for the advantage of others than for his own.

His assistance is gratefully acknowledged by many whom he has obliged in this way. This temper gained him many friends among the medical students—at the suggestion of whom, he commenced, in the spring of 1822, to give a six-months course of lectures on anatomy and surgery, perhaps one of the fullest courses of lectures ever given in this city. In this novel and laborious undertaking, which began immediately after the spring commencement, and lasted, with the exception of the month of August, till the ensuing November, six lectures being delivered every week, he was encouraged by a considerable class. In the progress of this year, he gradually acquired the habit of lecturing with ease and perspicuity—his enunciation, which originally was rapid and somewhat difficult to be understood, gaining a more

even flow. He at all times possessed, personally, the warm attachment of his pupils, and their high estimation of his talents—and to which, he was rapidly adding the elegance and facility of an eloquent lecturer. He was engaged in a second course of lectures of the same kind, at the period of his lamented decease.

In October, 1818, he married an amiable lady, by whom he had an only daughter—both of whom survive him.

Dr. Lawrance's principal medical merit was the prosecution of morbid anatomy. The opportunities for this pursuit in Philadelphia, are very great, and which he embraced to the fullest extent. He was in the constant habit of recording facts and observations of every kind, relating to medical science, which occurred in his daily pursuits, and particularly accounts of dissections. His accumulations of this nature rose to an immense amount—and an index was carefully kept, referring to every case. This was the common employment of those hours which he uniformly stole from sleep. By these means he became, probably, the best qualified among our American physicians, to publish one of those useful works on morbid anatomy, which do honour to the names of their authors, and form, in fact, the greatest and surest support of medical knowledge. The American public, however, never patronises the sale of such works sufficiently to authorize their being printed—and it is doubted, notwithstanding Dr. Lawrance's great collections, whether he ever designed an ultimate publication.

During the two summers preceding that of his death, he was engaged, with some of his medical friends, in a course of experiments of considerable length, with a view to elucidate the subject of absorption, and in particular, the recent discoveries and doctrines of the celebrated Magendie. These experiments, on their publication, were noticed in favourable terms, by several Journals, both at home and in England. In these investigations, his assiduity and operative skill were eminently remarkable—contributing in a principal degree to their amplitude and success.

His inaugural thesis, which, in compliance with the cus-

tom of the time, was not printed, was upon fractures of the thigh—a subject which he treated from observation in the hospital and elsewhere, with a candour and caution in stating the results of different modes of practice, highly creditable to his feelings and principles.

He was, we believe, bred a Roman Catholic—though upon the subject of religion, he maintained, in his conversation, a reverent silence. He did not deem it a fit theme for discussion in mixed companies. His friends, however, knew that he had a tolerance for all, nor thought that belief in any particular creed was a part of the necessary duties of man in this world, or of the commands of the Creator. In all the duties of social life, he was truly exemplary.

Dr. Lawrance was a member of the American Philosophical Society, and of the Medical Associations existing at the time among his equals in age. Besides the two copious courses of lectures mentioned above, he delivered the greater part of another on anatomy, during one winter, at the University. He had been for about a year, a surgeon to the Philadelphia Alms-House.

Such was the unostentatious life of one, who would probably, in a few years, have become a light of the age. In merits solid, as in disposition benevolent and kind, though his worth may not be known to future times, it will be deeply felt and remembered during a period co-equal with the life of his friends, his fellow labourers and his pupils.

Quando ullum invenient parem!

B. H. C.

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

ANATOMY AND PHYSIOLOGY.

Motions of the Eye.—A paper, in illustration of the uses of the muscles connected with the eye-ball, was lately communicated to the Royal Society, by Mr. Charles Bell. The object of these inquiries is to show that certain motions are performed by the eye, which have not hitherto been described. Thus, every time the eyelids are brought together to cover the globe, the eye turns upwards; without which motion it would not be properly moistened, nor the particles of dust removed from its surface. Next it is shown, that during sleep the ball is turned up, so as to lodge under the superior palpebra. These movements are rapid and involuntary, while others are under the government of the will, and for the purpose of directing the eye to different objects. Both the oblique and straight muscles have been regarded as voluntary; but Mr. Bell maintains that the oblique are for the performance of the insensible motions, and the recti muscles for those under the command of volition. According to this view, we are enabled to judge of the distance and relative position of objects by the consciousness we have of the action of the straight muscles. A further inference is, that the functions of these muscles are connected inseparably with the state of the retina, being active only so long as the sense of vision is in activity: when this ceases to act, the oblique muscles come into play, and draw the pupil upwards beneath the upper eyelid. Hence, that turning of the eyeball which we witness in sleep, in fainting, or in death, is but the indication of insensibility. In another paper, the author proposes to explain the uses of the numerous nerves going to the orbit, on the principles of the foregoing remarks.—*Lond. Med. and Phys. Jour.*

On the Transition of Liquids from the Mother to the Fœtus.—From experiments on pregnant rabbits, professor Meyer concludes, 1st, That stillicidious fluids, infused into the mother, may be tranferred to the fœtus, though he does not deem the point to be quite settled, more comparative experiments being wanted to establish it; 2d, That the transition takes place by

the deposition of the fluid in the cavity of the amnion; 3d, That the lungs and trachea of the fœtus exhibit no traces of the injected fluid, which proves that the fœtus does not, as Scheel and others maintain, draw the liquor amnii into the lungs. M. Meyer never thought this opinion sound, and explains the appearance of animal liquor in the trachea of a fœtus, by observing that the fœtus sometimes makes respiratory movements while enclosed in the membranes of the ovum, which may also take place with the mature fœtus in utero, if the circulation be any way interrupted. This, however, is not natural, and may prove fatal to the fœtus.—*Hamburger Archiv. für Medicin.*

Experiments on the Pulse.—Our readers may compare the following remarks with the theory of the late Dr. Parry, which has excited so much notice. M. Dollinger, on visiting some water-works, observed, that on grasping the lead pipes into which water was forced upwards by pumps, he could plainly perceive an impulse at each successive wave, very much resembling what is felt in the arteries. As this could not arise, as he ascertained, from the trembling of the pipes, which were well secured, nor from the vibrations of his hand, he was anxious to find whether the same effect would be perceived in an artery when laid bare. In the presence of his pupils, accordingly, he laid bare the carotid of a dog, and though no motion could be seen by the eye, the pulse was distinctly felt. He hence concluded that the arteries suffer no alternate contraction or dilatation. The pulse is therefore, he thinks, produced by the wave of blood, the impulse being communicated without the artery suffering any dilatation, in the same way as the last of a series of elastic balls is moved by an impulse communicated to the first, while the intervening ones are unmoved.—*Magazin der gesammten Heilkunde.*

Blumenbach on Irritability of the Tongue.—I had the tongue of a four year old ox which had been killed in the common way, by opening the large vessels of the neck, cut out in my presence while yet warm, and at the same time, the heart, in order that I might compare the oscillatory motion of this organ, which is by far the most irritable that we are acquainted with, with the motion of the tongue; and, when I excited both viscera at the same time, by the same mechanical stimuli, namely, incisions with a knife and pricks of a needle, the divided tongue appeared to all the bystanders to survive the heart more than seven minutes, and to retain the oscillation of its fibres altogether for a quarter of an hour; and so vivid were the movements when I cut across the forepart of the tongue, that the butcher's wife compared them to those of an eel in similar con-

dition, quite in the way that Ovid has compared them to the motions of the tail of a mutilated snake.—*Edin. Phil. Journal.* VIII. 263.

Mr. Henry Earle on the Mechanism of the Vertebræ.—In birds, the mechanism of the spine, and the spinal canal, is so contrived as to allow a remarkable extent of motion in the neck, without the medullary column suffering from pressure. It is a very singular fact, that in birds, the cervical vertebræ are numerous; but that the number varies from nine to twenty-four, while among mammalia, there are uniformly seven, except in the three-toed sloth. He exemplifies this in the short-necked mole, in the horse, and in the giraffe, whose neck is, we believe, about four feet long.—*Phil. Trans.* Pt. II. 1822.

Mr. Wilson's Comparative Anatomy of Trochilus Colubris.—On dissection, the heart of the common humming-bird was found to be remarkably large, nearly as big as the cranium; and the stomach, even when distended, uncommonly small, not exceeding the globe of the eye, and scarcely more than one-sixth part as large as the heart. The fibres of the heart were also exceedingly strong. The brain was in considerable quantity, and very thin. The tongue was perforated from the tip, the whole extent of the bill, forming two closely attached cylindrical tubes.The other extremities of the tongue corresponded exactly to those of the woodpecker, passing up the occiput, and reaching to the base of the upper mandible. These facts were verified in five different subjects; in all of whose stomachs were found insects, either whole, or in fragments, so that the bird does not live solely, if at all, on honey, or the juice of flowers.—*American Ornithology.*

Dr. Reisseisen on the Structure of the Lungs.—In an Essay on this subject, our author was considered equal to Sömmerring, who was at the same time candidate for a prize offered by the Berlin Academy. He proved, contrary to Helvetius, and in some degree accorded with F. Meckel, that the lungs are a very minutely divided extention of the trachea, the branches of which gradually lose their cartilaginous portion as they divide, and continue their course till they end, not in cellular membrane, but in a simple membranous cul de sac. Over this a vascular plexus is spread, arising from the bronchial arteries, out of which the pulmonary veins at once spring; and of course, the arterial blood is mixed with the venous blood. The bronchial veins, again, do not form a trunk, but open separately into the pulmonary veins, which return the blood to the heart. He thinks he has demonstrated, in opposition to Haller, that

the lungs are well supplied with nerves, not from the intercostal, but from the pneumo-gastric; and he denies all anastomosis *in the lungs* with the eighth pair. He considers the state of collapse as the natural state of the lungs.—*Saltzburger Zeitung Medizin und Chirurg.*

M. Itard on the Transmission of Sound by the Eustachian Tube.—M. Itard controverts the received opinion, that sound is partly conveyed to the ear through the inner opening of the Eustachian tube. The experiment upon which the common opinion is founded is, that when a watch or other sounding body is made to touch the teeth, it is heard more distinctly. It is unfortunate, M. Itard thinks, for this theory, that by putting the watch back on the tongue, without touching the teeth, and at the same time, shutting the mouth, the sound cannot be heard at all, though the watch is much nearer the Eustacian tube. We have tried this, and find that M. Itard is correct. But why then, it will be said, do people open their mouths, when listening attentively? Perhaps to allow the sound to strike on the teeth, which are good conductors.—Itard, *Maladies de l'Oreille.*

M. Flourens' new Experiments on Irritability and Sensibility.—The researches of this promising inquirer, in some measure tend to illustrate the interesting experiments of Bell, Shaw, and Magendie, of which we recently gave an account, and in some circumstances carry our knowledge still further. M. Flourens lays claim to more precision in his experiments, than former inquirers, whom he blames for not stating on what particular parts of the brain they produced pressure, or what parts they penetrated. In one experiment, (which was often repeated) he cut away the cerebellum of a pigeon in successive layers. It remained spirited, erect, and could see and hear to the last; but the slicing successively produced weakness—hesitation—unsteady agitation—inability to walk and to stand upright; and at last it remained on the back or the belly, but still restless, though not convulsed, so long as the spinal marrow or the tubercula quadrigemina remained untouched. The power over the voluntary muscles seemed to be the only loss. This is one of his most interesting experiments.

In another experiment, frequently repeated, M. Flourens removed the right lobe of the cerebrum from a pigeon. It instantly lost the sight of the left eye, though the contractility of the iris was not affected; and the right side of its body appeared feeble, though it was otherwise well, and could stand, walk, run, and fly. When the other lobe was removed, the sight of the left eye was lost, though the iris remained contractile; but general debility and stupor succeeded, and volition, memory,

hearing, and all perception, seemed to be lost; yet it walked, when pushed, and flew, when tossed into the air. When the cerebrum was left entire, and the tubercles removed, the loss of vision was produced by destroying the contractility of the iris, as the tubercles are, with the medulla oblongata and the spinal marrow, centres of the nerves of motion. The removal of one of the tubercula, produces in the eye of the opposite side blindness and involuntary rotation. The limits, therefore, of exciting muscular contraction, are fixed at the tubercula, and never extend to any part of the cerebrum or cerebellum.--CUVIER's *Report to the Acad. of Sciences.*

Pathological Phrenology.--We are glad to learn that Phrenology is at last going into tangible and practical investigations. The discovery which we have to announce regards the effects of the cerebral organs on the osseous structure:—namely, when any organ is highly developed or is in great activity, it *thins the bone* on which it impinges. A man in the Dublin Hospital, who had the organ of talkativeness so strongly active that he never ceased from talking except when asleep, happened to die; and on *post mortem* examination it was found that this active organ had rendered the bone over it as thin as paper and nearly transparent. We marvel exceedingly, that some of these very active organs do not occasionally get through the bone altogether and escape into open day: Bonaparte's organ of ambition for example. Quere, whether in such cases it might not be advisable to prevent such a catastrophe by employing the operation of trepan and removing a portion of the bone-thinning organs? —*Quar. Journ. of For. Med. and Surg.*

M. Fodera on Absorption.—A paper recently read to the Institute, contains the result of various interesting experiments on absorption and exhalation, by M. Fodera. The object of this physiologist is to show that exhalation, which he denominates transudation, and absorption, which he calls imbibition, are in reality the same phenomenon, arising from the imbibition of different vessels, operating in the first case from the interior of the vessel outwards, and in the second from the exterior inwards. Majendie had been led to conclude that venous absorption was effected by imbibition; and one of the experiments leading to this opinion was that of isolating a portion of a venous trunk, and placing its surface in contact with a poison: the presence of this within the vessel was soon manifested. M. Fodera has reversed this experiment. He injected a poisonous substance, with all necessary precaution, into a portion of artery, confined between two ligatures, and isolated, not only from the cellular texture, but likewise, he informs us, from the lymphatics and

vasa vasorum: the poison took effect. He obtained the same result on filling a portion of an artery, vein, or intestine, with the poison; removing them, and placing them either in a wound made in another animal, or in the abdominal cavity. The rapidity of the poisoning in these cases varied according to the age and species of the animal, the thickness and length of the portion of vessel or intestine used, its more or less complete distention, and the degree of solubility of the poison. The same phenomena presented themselves when sulphuretted hydrogen was employed.

If an artery or vein be laid bare in the living animal, an oozing is observed to take place through the coats of the vessel. This oozing is increased if a ligature be applied, and dropsy may be produced in this manner. From these facts, M. Fodera concludes that exhalation is only a transudation through the vascular parieties; and many physiologists thought so before the existence of exhalant vessels was establihsed.

M. Fodera has likewise devoted considerable ingenuity and research to the explanation of the phenomena which attended the rapid passage of various substances from the stomach to the bladder, by which it appears that the experimentalists of this country were mistaken in supposing that such transition was effected by some other than the ordinary medium of the lymphatics or blood-vessels.—M. Fodera introduced a catheter, with a cork adapted to it, into the bladder, and then injected a solution of prussiate of potass and iron into the stomach; as soon as the salt was detected in the urine, (an occurrence which in once instance took place in ten, and in another at the end of five minutes,) the animals were instantly opened, and the prussiate was found in the blood of the vena cava inferior, of the heart, and of the aorta, in the thoracic duct and other parts. These experiments, if found by others to yield similar results, must be admitted as proving both the extreme rapidity of absorption, and that the communication between the stomach and bladder must be looked for in the usual course of the circulation.

The celerity with which absorption is effected in some organs, is rendered still more striking by experiments upon the lungs. M. Fodera injected prussiate of potass into the trachea, and cut out the heart of the animal (a rabbit,) as soon after as possible. The operation was performed in twenty seconds; and, notwithstanding the shortness of the time, the interior of the left auricle was stained of a greenish-blue colour, which was deeper in the mitral valve, and less apparent, although still perceptible, in the aorta. It will be observed that the results detailed by M. Fodera agree, generally, with those obtained by Majendie.—*Med. and Phys. Journal.*

MORBID ANATOMY.

Dr. Peter's Case of Perforation of the Stomach.—A woman, aged 52, who had been gouty for eight years, and subject to hysteria and hemorrhoidal flux, was seized with sharp pain in the left hypochondria. After eating she had acid eructations, accompanied with retching and a dull pain in the stomach. The primary pain at length became so very severe as to deprive her of sleep, and in the course of a week she sunk under attacks of incessant convulsion. On dissection, a quantity of very fœtid gas escaped, and in the cavity of the abdomen about four pounds of a yellowish liquid was found. There was afterwards discovered towards the middle of the small curvature of the stomach, a perforation sufficiently large to admit the little finger. The liver and other viscera were healthy. The right ovarium was much enlarged, and traces of a fœtus were found, which was the more remarkable as she was not known to be pregnant.—*Hufeland's Journ. der praktischen Heilkuude.*

Singular Case of Internal Hemorrhage.—A woman aged 33 years, who had always regularly menstruated, had her menses suppressed, in consequence of catching cold, and getting her feet wet. She fell ill the same evening, and died at noon, on the following day. On examination, no traces of inflammation could be discovered in any part of the body; but, in the abdomen, upwards of three pints of fluid blood were found, without any evident cause being traced for such an occurrence. It is needless to state, that the accident was attributed to the sudden cessation of the menses.—*Hufeland's Journal.*

Dr. John Davy on Post Mortem Effusions of Serum.—It is a subject of considerable interest to determine whether the effused serum, so often found in dissecting, has been poured out previous to death or after it. To determine this point Dr. Davy suddenly killed several dogs by a blow on the occiput, and immediately laid open the pericardium, when a quantity of serum found there was removed with a sponge, and the incisions were closed up by sutures. Twenty-four hours afterwards the pericardium was again examined, but in no instance was a single drop of serum found, though the heart was distended with blood.—*Phil. Trans.*

SURGERY.

On the Russian Treatment of Hernia.—The following mode of replacing incarcerated hernia is employed by the Russian peasants. They take a vessel capable of containing a few pints, make a hole in its bottom, and stop it with a cork. The parts about the tumour are then rubbed with oil or soap, and the air in

the vessel being rarefied with lighted tow, is placed over the parts like a cupping glass. The abdominal parietes are of course forcibly pulled into the vessel, while the tumour is drawn into the abdomen. The vessel is then removed by withdrawing the cork, and admitting the air. M. Hildenbrand has tried it repeatedly with great success, premising blood-letting, where inflammation is present. It cannot be used in cases of pregnancy, dropsy, or much corpulence. The Russians, likewise, employ the same process in uterine hemorrhage, and spasm, which they refer to a wrong position of the uterus.—*Svenska Läkare Salskapeto Handlingar.*

Treatment of Nasal Polypus.—The operation of extirpating a nasal polypus is not a very gracious one, nor always successful; and we therefore mention the following as doing away with its necessity, *si probatim sit.*

M. Mayer says, that the *marum verum* of Linnæus powdered, and taken as snuff in the quantity of five pinches a day, is effective in removing polypus of the nose. The powder is very astringent, and produces evacuations of blood from time to time, and ends by destroying the polypus. M. Mayer gives the case of a man who had had a polypus of the nose from the age of fifteen, and which, in spite of repeated extirpations, continued to recur, though he had consulted all the best surgeons in Germany, France, and Italy. The powdered *marum* produced a complete cure, after being used as snuff for a short time.—*Hauffland's Journ. Berlin.*

Dr. Arvend's Cases of Ligature of the External Carotid, and External Iliac Arteries.—Cases of this kind are rapidly multiplying, and the bold surgery of England is making its way even to the barbarous regions of the North. Dr. Arvend, principal surgeon to the Hospital of Artillery in St. Petersburgh, has related two cases in the Imperial Conservator, in which he severally passed ligatures round the external iliac, about two inches below its exit, and upon the right carotid, an inch above the clavicle. Both operations were successful. Both patients were about the same age of forty-four. The ligature of the iliac canal came away on the sixteenth day, that of the carotid on the seventeenth.—*Salzburger Zeitung, Med. und Chir.*

Mr. Travers's Case of the Ligature of the Subclavian Artery.—In a patient, upwards of sixty years of age, in St. Thomas's hospital, Mr. Travers, assisted by Mr. Green, tied the subclavian artery, Jan. 17th, 1823. Mr. Travers made an angular incision, by letting fall a perpendicular one along the outer edge of the sterno-mastoid muscle, on the first incision made above

the clavicle, and then proceeded to dissect from the flap. The great elevation of the clavicle rendered the operation very difficult, and there was a great deal of hemorrhage. The operation occupied upwards of two hours. The patient died the second or third day.—*Med. Chirurg. Mag.*

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*M. Wolfe's Operation for Imperforate Anus.**—The subject of this operation was a male infant, who came into the world of a weak form and unhealthy hue, altogether resembling a 7th or 8th month's child. The imperfection in the anus was not discovered till the evening of the 12th day from his birth: during this time he was restless, cried much, and was at last affected with hiccup and convulsions. M. Wolff, on being called in, found the abdomen protuberant, hard, and painful to the touch; there were also nausea and vomiting, with great depression of strength. He instituted an examination, and discovered the cause of the symptoms. Next day he performed the operation by pushing in a large lancet into the middle of the perinæum, a few lines distant from the os coccygis, he passed it an inch deep towards the sacrum without meeting with the rectum. After stopping the bleeding he renewed the puncture, but without effect, although nearly two inches deep. He felt, however, the bladder through the wound, and also the hypogastric arteries pulsating. He had now recourse to a pharyngotome with a bent and somewhat broad tube. This was pushed fully two inches deep, and entered the rectum: a clyster was immediately administered, which brought away some meconium. By the use of glysters and setons retained in the wound, the child rapidly recovered, so that by the 7th day from the operation the discharge per anum followed naturally, and when seen some time afterwards it was in good health, and free from any disturbance in the functions of the rectum. In cases where the imperforation is of the kind described, Mr. Wolff prefers the pharyngotome to the trocar and lancet.—*Langenbeck's Neue Bibliothek für die Chirurg.*

M. Itard on injecting the Internal Ear.—The instruments requisite to perform the injection properly, are a syringe, a silver tube, an elastic gum tube, and a silver frontlet, to be employed as its name implies. The success must depend on the anatomical skill and dexterity of the operator, and he must rely, in an eminent degree, on his habit of introducing it in the dead subject for success and facility in employing it in the living body. M. Itard fixes the frontlet first, then measures the distance be-

* Glücklich verritete operation eines verwachsenen Mastdarmes. Vom Landchirurgus E. Wolff in Celle.

tween the superior alveolar margin and the basis of the uvula, which is nearly the same with that between the posterior commissure of the nostril and the guttural orifice of the Eustachian canal. This measure is to be marked on the silver tube, which is then to be introduced into the nostril on the side corresponding to that tympanum which the surgeon wishes to inject. Its entrance into the canal, which is to be known only by the most comsummate *tact* acquired by the frequent habit of introducing it, both in the dead and living body, will enable the operator to fix its other extremity immoveably between the limbs of a *vis* or screw attached to the frontlet, and proceed to the adaptation of the syringe. All after this is simple and easy.—*Maladies de l'Oreille.*

M. Amusat on Fracturing Calculi in the Bladder.—This is one of the most novel of the many methods which have been proposed to get rid of calculi, without having recourse to the dangerous operation of Lithotomy. M. Amusat has invented an instrument, consisting of pincers confined within a tube, not larger than a sound; by which, when introduced into the bladder, the stone can be easily seized, reduced to powder, and voided as gravel. A stone, the size of a nut, can be pulverized in a few seconds. It has as yet been only tried on the dead body.—*Bibliotheque Universelle.*

M. Grafe's Excision of the Lower Jaw.—Professor Gräfe, of Berlin, has removed one half of the inferior maxilla from its articulation; in consequence of which operation, it became necessary to apply a ligature to the left carotid artery. The patient was a girl twenty-three years of age, who had a bony tumour situated on the jaw ever since she was six years old, which had then grown to an enormous size. The patient supported the operation with great fortitude. The wound was dressed on the fifth day, when the patient was not only able to utter some words, but to take food without great difficulty.*—*Salzburger Zeitung.*

THEORY AND PRACTICE OF MEDICINE AND MATERIA MEDICA.

M. Esquirol's Account of a Colony of Maniacs, at Gheel, near Brussels.†—From time immemorial, there has existed in the

* We believe that more than one case of this kind has been successfully treated by a similar operation, by that distinguished surgeon Professor Mott, of New York.—Editor.

† Notizie sul villagio di Gheel. Del Signor Esquirol. (Lette all'Accademia Reale di Medicina.)

[We have not seen the French paper whence the Italian one is taken.—Editor.]

centre of Belgium, in the Commune of Gheel, a colony of maniacs, of which little has hitherto been accurately known by the public, or the profession, till M. Esquirol visited it in 1821. The first public notice which we find of them is in 1803, when the Prefect of Dyle caused all the maniacs kept in Brussels to be taken to Gheel, as they were badly accommodated in the hospital at Brussels. The following year, a short notice of the state of Gheel, was inserted in M. Herbouille's Statistical Account of the District; in which, he says, that this "strange traffic has been, time out of mind, the only resource of the inhabitants of Gheel, and no accident from it was ever known to have taken place." Dr. Andrée is the next writer who mentions Gheel, in his work on Charitable Establishments, published in 1808. He was informed, that madness is endemic at Gheel! adding, that he had not time to examine "Ilo fondamento di questa voce popolare;" but if it be true, it will be an interesting physiological phenomena. A still more erroneous account is given by M. Jouy, in the third volume of the "Hermite de la Chaussé d'Antin." Even the Geographical Dictionary of the Pays Bas copies verbatim, Herbouille's Statistical Account.

On the 29th of August, 1811, M. Esquirol went to Gheel, accompanied by Dr. Vaisin and M. Vanetzbon, director of the Belgic Mint, who undertook the office of Flemish interpreter. He remained two days, walking about the village, visiting the inhabitants, and investigating the peculiarities of this singular establishment. The small town of Gheel, is situated on the north angle of a triangle, formed by Anvers, Malines, and Gheel, and with some hamlets and farms in the vicinity, contains about six or seven thousand inhabitants, of which four or five hundred are maniacs. It has only a single street, which is broad and paved: the houses are well built, but have generally but one floor. He met a maniac in the street, who was polite enough to conduct him to the parochial church, the hospital, situated near the centre of the town, and to the church of St. Amanzius, whose architecture bespoke it to be of the thirteenth or fourteenth century. Here is the shrine of St. Nymphna the Martyr, whose bones were miraculously discovered in the seventh century, and to whom the colony seems to owe its existence; for it being discovered that St. Nymphna had the peculiar power of curing maniacal distempers, her shrine was soon crowded with devotees. Maniacs were accordingly brought thither in great numbers, accompanied by their relatives, and the inhabitants found it lucrative to board and lodge the strangers.

St Nymphna still continues to enjoy her celebrity for the cure of mania, but her credit is, as might be expected, rather on the decline; for the old rector, who is upwards of seventy, admitted, that though he had frequently seen cures effected by the

intercession of the saint, yet these were becoming daily more rare. To obtain a cure in this way, a very great number of minute, tedious, expensive and absurd ceremonies must be gone through, which it is unnecessary to detail. The following facts are more interesting.

The relatives of the patients intrust them to the inhabitants of Gheel, under a sort of contract. The vicinity of the church is in most request for patients, though some are lodged in the neighbouring farms and hamlets; but M. Esquirol met with few beyond the town. Each inhabitant may take from one to five patients, and for the poor of the commune an hospital is provided, in which eight or ten are received. The patients who are mischievous or unruly, sleep apart upon straw, or on a bag of chopped straw. Those who are more harmless have similar beds to their hosts, and eat at the same table; and of course, those in town, though they have not so good air, have better food and better beds, than those lodged in farms and hamlets. The patients maintained at the expense of the hospitals of Brussels and Malines, are clothed in woollen stuff; the others, according to the fancy of their relatives. The greater part of them live, like the other inhabitants, on milk, butter, and potatoes. They are allowed to walk in the street, or in the country, without restraint, without fear, and even without being mustered. When they escape beyond the territories of the commune, they are pursued by the gens d'armerie, and conducted back to their homes. When any of them become unruly, they are loaded with irons, both on the hands and feet; and M. Esquirol saw one poor fellow walking in the street with his legs much lacerated by the friction of his irons. Many of them are employed to the great advantage of their hosts, in agriculture, and other simple labours. The female patients are all employed in sewing and making lace, and are exempt from all domestic services. A very small remuneration is given for such services, such as a flask of beer on Sundays, &c. The patients are not allowed to go to the parish church, but fifty or sixty of them assist in singing, and in various parts of the service, at the church of St. Amanzius. Sometimes they will interrupt the service, but this is rare. The order of the police prescribes that none of the patients must be seen out of doors after sunset, and that none who are furious be seen out at all. Charities pay from two to three hundred francs per annum for each patient; families pay from six to twelve hundred francs.

The administration of Brussels maintains a director at Gheel, who has officers under him, forming, with two physicians, a commission of surveillance; pregnant female patients are sent to Brussels, but this rarely occurs, being only in the proportion of five in ten years. He learned from Dr. Backer, who has

practised at Gheel for thirty-two years, that the patients are generally incurable; suicides are rare; thirty years ago, a patient cut his throat in the church, during the nine days ceremony for his cure. There are more cures made in the suburbs than in the town, though it is remarkable that the patients are worse treated.

Among the most prevalent causes stated by Dr. Backer, are religious melancholy, deluded ambition, unsuccessful love, and domestic misfortunes. The most hopeless cases are those arising from religious causes. Intermittent mania is frequently cured, when the patient can be induced during the sane intervals to engage in rural labours. Monomania is sometimes successfully treated with neutral salts in aqua graminis. Vinegar is thought useful in restraining fury. The mortality among the patients is a little more than that of the other inhabitants; the females in particular, are subject to a diarrhœa of black bilious matter, which often proves fatal. These two last years, the number of patients has been about four hundred, the females being nearly in the same proportion as the males. About three fourths of the whole have full liberty to go and return at their pleasure; and they are never crowded round and tormented by the children, as would be the case in other places, and the inhabitants live in the midst of them with perfect security. M. Esquirol has transmitted proposals to improve the establishment at Gheel, to the minister of the interior of Holland, and we only wish that these improvements, if adopted, may not be—as has too often occurred in such cases—changes for the worse.—*Quart. Journ. of Med. and Surg.*

Waters containing Carbonic Acid.—It results from many observations made by M. Desportes, that the use of Seltzer water, either natural or artificial, the water of Pougues, or, generally speaking, all those waters containing carbonic acid gas, should not be lightly recommended, and that the idiosyncrasy of patients should previously be consulted; and that the first effects of such waters prescribed during meals, mixed with wine, should especially be observed. The author has seen the action of these waters derange the digestive functions, and, by sympathy, produce an irritation of the brain, which put on at first the character of intoxication: with several patients, severe nervous symptoms and a threatening of asphyxia were the consequences, and particularly violent pains in the region of the heart.—*Revue Med.*

Case of Diabetes, treated with Carbonate of Ammonia. By Dr. Neumann, of Berlin.—A female, aged forty-eight years, of a feeble and hysterical constitution, suffered an attack of abdo-

minal dropsy. This disease was soon after followed by an abundant secretion of urine, and a voracious appetite, with extreme emaciation. The diabetic state of the urine was demonstrated both by its quantity, and by its saccharine properties. Carbonate of ammonia was prescribed, and continued for four months; its dose was increased from five grains thrice daily, to fifty grains in the day. The patient left the hospital perfectly cured.—*Medical Repository.*

On the Cure of Intermittents by Frictions with the tartarized Antimonial Ointment on the Epigastrium.—During the winter of 1815, Dr. Prommer had occasion to treat numerous cases of intermittents occurring in the army of Wurtemburg, cantoned at that time on the Loire and Allier. He frequently found the cinchona to fail in producing its usual effects, and observed that the fever generally disappeared on the eruption of pimples or pustules on any part of the body. Taking advantage of this observation, he was induced to try the effects of artificial eruptions in its cure. With this intention he employed the common tartar emetic ointment, and prescribed it to be rubbed upon the abdomen and epigastric region. The first two individuals whom he subjected to this treatment, recovered immediately upon the maturation of the pustules which were thus produced. This induced him to use the same means in a great number of cases then under treatment, and in those which subsequently occurred, and in all the cases with similar success. Dr. P. employed the same mode of cure in those varieties of agues which were complicated with nervous symptoms with the same benefit.—*Journ. der. Praktischen, Heilk.* 1823.

Vaccination.—A child, aged about twelve months, was vaccinated in the arm. No inflammation followed. In about eight or ten days therefrom, the operation was repeated, and on the same arm. This attempt was successful, and the child passed regularly through the disease, the first puncture remaining all the time quiescent.

In about *six months*, however, this puncture inflamed spontaneously; and the physician who was called in, asserts, that he found on the part a well formed vesicle, "presenting in every way the satisfactory appearances we expect to find about the tenth or eleventh day after the insertion of the vaccine lymph."

He adds, "the cicatrices of both pocks now remain, and are equally well defined."—*Lond. Med. Journ.*

When Mr. Pearson transmitted to China some of the vaccine lymph, he sent with it a pamphlet, in the Chinese language, containing directions for its use.

Of this pamphlet an edition was published in China soon af-

ter its arrival, but expurgated; so that nothing appeared from which it could be learned that vaccination was not a Chinese discovery!!!—*Annals of Philosophy.*

Therapeutic Effects of Hyoscyamus.—"Until it shall be held absolutely necessary to procure a patient a rending headach, with vertigo, phantastic reveries, burning thirst, loss of sight, perversion of the taste, insuperable disgust for every sort of food or drink—without a single wink of sleep, this remedy must continue utterly useless; and in the mean time, it should be expunged from every Pharmacopœia."—*Archives Gén. de Méd. Mars,* 1823. Such is the conclusion to which M. Fouquier of La Charité has been led by experiments with the hyoscyamus on two hundred of his patients. He denies that it ever acts decidedly as a hypnotic. In opposition to his observations, we may remark, that for a long time it has been freely used in the Infirmary of this city for inducing sleep; and that, as far as we have ourselves observed, it is as effectual as opium itself, and never produces the effects observed at La Charité. The experiments of Fouquier tend to show, that the extract prepared from the expressed juice, and the aqueous extract procured by macerating the dried plant in water at 100° F., and evaporating the product by the water-bath, are nearly or absolutely inert. But M. Planche prepared for him an alcoholic extract of great power, by maceration in weak alcohol at 82°. Of this extract he seems to have given from ten to thirty grains for a dose; and always observed it produce the disagreeable symptoms we have mentioned. The form under which it is used in this country is that of tincture, very similar to that from which Planche prepared his extract. Does the alcohol qualify its properties, and render it more purely hypnotic?—*Edin. Med. Surg. Journ.*

Syphilitic Inflammation.—Cases often occur in which inflammation appears of an obstinate, tedious, and erysipelatous character, without symptoms of a syphilitic origin, though it may be readily confounded with syphilitic inflammation, particularly where the central ulcer is not sufficiently formed, or too deep in the throat to be discovered. In these cases Dr. Von Autenrieth never met with the following symptom, which, on the contrary, he never found wanting in any case where a common syphilitic ulcer, either large or small, superficial or deep-seated, existed in the throat. This characteristic symptom is a colourless watery froth, consisting of little vesicles found in large or small quantities at the hind part of the root of the tongue. Wherever he found this froth, he was sure to meet with a syphilitic ulcer if ever so hidden. A very close examination at the moment, when, on the root being pressed down, the patient in-

voluntary attempts to swallow, easily discovers whether the froth be present. By attending to this, the author was never deceived in his diagnosis.—*Mag. Auslandischen Literature.*

Proofs that the Yellow Fever of Spain is different from that of the West Indies.—We are informed, by a great number of physicians who have practised in the West Indies, that the *yellow fever* in that part of the world is endemic, and not contagious, and that it is possible to escape its influence by retiring into the interior of the country, and leaving the low, damp, and marshy sea-coast, where it is produced; that it never spreads in those elevated and healthy districts, even although persons ill of the fever should be brought there; that the natives, and those who have become accustomed to the climate, are rarely influenced by these local causes; that those persons who inhabit the mountains run nearly as much risk from the disease as Europeans, when they visit the low grounds; but that Europeans are in a more particular manner the victims of the disease.

We can hardly withhold our assent from these conclusions, which are supported by such strong testimony, and so many circumstantial facts; but it would be very dangerous to apply the doctrine of endemic and non-contagious causes to our continent, and more particularly the Spanish peninsula. It is proved by the following facts, that the yellow fever of Spain has a very different character, which is, perhaps, occasioned by the climate of Europe. These facts I collected on the spot.

1. Its mortality is greatest amongst the natives.

2. It is not confined to the sea-coast, but spreads far into the interior of the country, and even into the driest, most elevated, and healthiest districts; for instance, to Carmona, which is from twenty to twenty five leagues distant from the sea.

3. It is found in the interior of the country, when the sea-port towns are free from it; as in Murcia, in 1812.

4. It was not produced in Spain, as we are assured it has been in the West Indies, by the effluvia of marshes, which had been rendered active, by exceedingly warm weather. The towns in the bay of Cadiz, where for the most part it first shows itself, are in general salubrious; at Chiclana, in particular, the rich inhabitants of Cadiz have their country-seats and pleasure-grounds.

5. It is believed by all the inhabitants of the country, that the disease is imported from America, and that it has been propagated by contagion over a great part of Andalusia. It must have been in consequence of numerous proofs of contagion, that lazarettos have been established, even in the cities of the interior.

6. The places where the disease is prevalent are not fatal to

strangers; for not a single Frenchman stationed in 1810, 1811, and 1812, in the situations where it usually appeared, was affected by the disease, which, besides, had not previously appeared for several years; nor did it show itself for several years after. Is so long an interval of its non-appearance consistent with its production from local causes inherent in the climate? If these had not been imaginary causes, they would surely have affected in a very violent manner those Frenchmen not habituated to the climate, who in the greatest heats of 1811, (the year of the Comet) were encamped on the sea-coast, on very low grounds, where a great number of mud-forts were built, which surrounded the whole bay. The hulks, so unhealthy, and so crowded with French prisoners, which had been stationed for several years in Cadiz bay, have never produced a single instance of yellow fever, although scurvy and dysentery have raged with the greatest violence on board of them.

7. It prevailed, in 1811 and 1812, but not in a very malignant form, in the kingdom of Murcia; and its being concentrated there was doubtless occasioned by the line of military posts which had been established to prevent its importation into the west of Andalusia, and by the difficulty of communication in time of war.

8. In the general retreat which took place in 1812, the army of Andalusia passed through the kingdom of Murcia, in the internal parts of which were some infected towns; but the bulk of the army escaped from the contagion, in consequence of strict orders having been given that no person whatever should enter them. However, the division of General Conroux was attacked by it, on account of one of the soldiers having had communication with a woman, infected with the yellow fever; but the rest of the army was preserved from the contagion, as Conroux's division was kept in the rear, in a state of complete insulation.

The following account of that circumstance, as it was put into the orders of the day, I received from M. Cohen. "The orderly dragoon of general Conroux enters a small town, when the yellow fever is prevalent, he has connexion with an infected woman, and is seized with the complaint. The general's aid-de-camp, and a few other men, are infected by him. The division soon arrives at Hiecla, a small town of Murcia, where there had been no epidemic malady, either amongst the inhabitants, or in the French battalion which had just left it. The infected patients of the division are placed in an hospital, and in a short time the aid-de-camp and ten men die, after having had the black vomiting."

From these facts, I think I may conclude that the yellow fever, which at that time reigned in Spain, was not produced by endemic causes. That it is independent of the high tempera-

ture of that climate; that it is equally prevalent among mountains as on the sea-coast, which is generally thought to be its seat; that it does not depend on climate, as natives and strangers are both equally liable to its attack; and that, as its progress has been arrested by insulation; and, on the contrary, as it has been produced, when that precaution was neglected, we have a proof of its contagious nature. Indeed, in consequence of the facts observed in 1800, and the following years, it is the general opinion of the medical practitioners of Andalusia, that the disease is contagious.—*Decadas Medico Quirurgicas.*

M. Porta, of Rome, on Cold Water in Poisoning with Opium.—We can now settle the chronology of Mr. Wray's practice in cases of poisoning with opium; though we have not the slightest doubt that Mr. Wray was quite unacquainted with Porta's case, which must have occurred at least six or seven years ago, though it is not dated. We give the case verbatim:—"Porta of Rome, relates the following most remarkable case of a poisoning with opium being counteracted with cold water. Peruvian bark being prescribed for a pregnant woman, labouring under hysterics, spasms, and faintings, to be taken internally in decoction, and the powder of it to be injected per anum, she received by mistake powdered opium instead of it, of which three ounces were taken in three days. The consequence was a costiveness, against which twenty-two glysters proved ineffectual, and a meteorismus threatened a speedy dissolution. This dreary event being already expected, one of the consulting physicians fell upon applying fomentations to the abdomen, cold like ice, and to inject glysters of a similar nature. The glyster being just injected, fæces smelling strongly of opium were discharged, and the patient recovered."—*Continental Med. Repos.*

Dr. Hufeland on Antimonials in Inflammation.—Before coming to this subject, M. Hufeland steps out of his way to attack our English practice in the exhibition of calomel, and we certainly think with a great deal of justice. We have heard Mr. Abernethy in his lectures say, that he would undertake to derange the health of a robust man by a simple dose of calomel, and yet we see it every day given to infants almost indiscriminately. From the time of Basil Valentin, who wrote the Triumphant Chariot of Antimony, it was used as the principal remedy in acute fevers. In the last century, Huxham founded his treatment of inflammatory diseases of the chest on his antimonial wine; and now Balfour, Hufeland, and others, seem to take credit for the discovery, or at least for the continuance of the remedy. Hufeland says, that under the form of tartar emetic, joined with nitre, ammonia, or simple oxymel, it will, "with

certainty," cure all inflammatory diseases of the chest! This is certainly too much, even for the editor of the Berlin Gazette of Health, or Magazine of Medical Wonders, as we may well call it. He is not pleased, however, with the plan followed by M. Peschier, of Geneva (*See Quart. Journ.* IV. p. 116.), who substitutes it for blood-letting and blistering. M. Hufeland, on the contrary, always commences with the abstraction of blood, general or local, according to the case, and does not, like M. Peschier, prescribe empirically, in all cases, from six to fifteen grains in six ounces of some vehicle.—*Journal der praktischen Heilkunde.*

M. Crane's Case of Boulimia.— A lady, aged twenty-six, had her appetite so morbidly increased that she took three or four pounds of meat at a meal, exclusive of bread and vegetables. She commonly vomited after each meal, and the ejecta were mixed with a glairy, albuminous, and sourish substance. Many physicians were consulted, and much medicine taken without the least effect. A continued fever at length supervened, and produced a complete disrelish for food; but as soon as it subsided, the boulimia returned as violent as ever. Dr. Crane inferred from this that there was a peculiar irritability of stomach which was increased by food. He therefore tried, but with no success, to confine the patient to liquid aliment of a mild nature, such as milk and arrow root. He next tried soups and nutritive enemata. This was more effectual, and he gradually allowed bread and other solid food. In six weeks, the appetite became natural, and has now continued so for nine years.—Boulimia is often caused by organic malformations. M. Landrè-Beauvais gives a case of a phthisical patient who had been boulimious all his life; he died, and on dissection it was found that he had no gall-bladder, and that the duodenum adhered to the liver. The intestines were unnaturally voluminous.—*Hufeland's Journ. and Diction. de Med.*

Dr. Yeats's Case of a Painful Affection of the Brain.—In February, 1819, a gentleman, aged forty, had symptoms of dyspepsia, which gradually began to affect the head. Laxatives, local blood-letting, and blisters, were tried, but with little relief, and the affection in the head increased, with deep-seated pain and heat, commencing at the back part of it, and becoming diffused over the whole cerebellum, often causing insufferable agony for two hours together. The crown and fore part of the head were not affected. The pain was most severe about four o'clock in the morning, and was aggravated by a horizontal position. On stooping he had confusion and giddiness. He would not submit to lose blood, as it already failed to relieve him. A seton

was therefore advised, and introduced by Sir A. Cooper. The patient was put on a low vegetable diet, and requested to keep *continually* in the erect position without going to bed, and to have his head, which was shaved for the purpose, unremittingly moistened with a cold lotion, consisting of a solution of muriate of ammonia, vinegar, and water. This was regularly persevered in for a week, with so great relief, that he was gradually permitted to sleep in a horizontal posture. The seton was kept in for two months. The only medicines taken were occasional purgatives, and super tartrate of potass as a diuretic to carry off the effused serum, which, according to Dr. Yeats, is always more or less present in the brain in such cases. The gentleman recovered, and continued well up to the last year, since which Dr. Yeats has not heard of him.—*Brande's Journ. of Science.*

M. Lavagna on Ammonia as an Emenagogue.—Since the time of Astruc, who asserted the rare success of emenagogues, it has been considered almost hopeless to try the medicines of this class, formerly in repute. M. Lavagna has endeavoured to revive the reputation of emenagogues by proposing the injection of ammonia into the vagina. He gives fourteen cases of amenorrhœa, in which this succeeded, sometimes in twenty-four hours, and at most in five or six days, not only to produce the discharge, but to remove the paleness, the oppression, the difficulty of breathing, the anorexia, the weakness, &c. The same success was experienced in all sorts of temperaments, plethoric, bilious, pituitous, and their complications. The proportion employed was ten or twelve drops of alkali, in two spoonsful of warm milk, often repeated in the course of the day. It generally produced in the vagina a sensation more or less painful, according to the strength of the mixture and the sensibility of the part; but in no case was any thing dangerous or troublesome produced.—*Biblioteca Italiana.*

M. Magendie on Strychnine and the Resin of Nux Vomica.—The cases in which strychnine may be used, are, according to M. Magendie, general and local diseases of debility, particularly in all kinds of paralysis. The mode of employing it is in pills, containing from a twelfth to an eighth of a grain, and it is necessary, to make them keep, to cover them with gold or silver. The formula is

℞ Strychnii purissimi gr. ij.
Conserv. Rosar. ʒss.
M. et divide in pilul. xxiv.

For the Tincture of Strychnine:—

℞ Alcoholis (36° cent.) ℥j.
Strychnii gr. iij.
Dose from six to twenty-four drops.

The resinous extract of nux vomica has similar properties to the strychnine, but it has the disadvantage of varying in its medicinal strength; whereas the strychnine is uniform. M. Edwards cured with it a case of amaurosis, complicated with paralysis of the superior palpebra. M. Magendie has seen it useful in impotency, incontinence of urine, and in drowsy debility and dyspepsia. A single grain, in form of pill, given at night, is the dose for producing tetanic symptoms, though it may be increased from four to thirty-four grains, according to the quality of the medicine, and the strength of the patient. To produce the milder effects, half a grain, or a grain daily, is the proper dose. The tincture is prepared thus:—

℞ Alcoholis (36° cent.) ℥j.
Extract. Sicc. Nuc. Vomicæ, gr. iij. [*Formulàire.*

Dr. Helenus Scott on the Guinea Worm.—Dr. Scott has given a very interesting account of the formidable dracunculus of the tropics. He says that it breeds in the moist earth during the rainy season, and that a medical friend of his saw a native gardener dig up a number of these worms on the coast of Malabar, of which he collected a number, and preserved them in spirits. There could be no doubt of the identity of the animal with that found in the human body. It is probable that the eggs or young of the Guinea Worm live in water, or moist places; and hence the Indians, who walk barefooted, are much infested with them. The cellular membrane under the cutis is its proper nidus; but it often goes deep into the cellular substance between the muscles, as in the socket of the eye, the mouth, the cheeks, or below the tongue. The first symptom of it is a small blister, which when broken or irritated, produces intolerable itching over the whole body, so as almost to drive the person mad. Dr. Scott, in this case, describes his own feelings while suffering under this intolerable itch. When the animal has been partly extracted, it has not the power of retracting itself, though it has of advancing—a circumstance of the greatest importance in practice; for if it is wound, as is often done, round a quill, and secured, it irritates the animal; and as it seems to have the power of communicating its disagreeable feelings to the patient, it is often productive of troublesome consequences. It is elastic, but it ought never to be stretched. The native surgeons try to trace its convolutions under the skin, and, cutting

down upon it, are often able to extract it at once, if in a soft part; but when it is on the hands or feet, and wrapped round a bone, this is impossible, and its extraction may be tedious, and even dangerous. It is best to draw out as much of it as will come easily, and cut it all off but about a quarter of an inch, over which a poultice may be put. This does not kill it, but it sometimes dies, and produces inflammation and abscess in the part. It will, of its own accord, come out slowly, like the hour hand of a watch. When extracted, it soon becomes stiff, and dies.—*Medico Chirurgical Review.*

M. Legras on Syrup of Acetate of Morphine.—Dissolve four grains of acetate of morphine in an ounce of distilled water, cold; and add fifteen ounces of common syrup. The transparency will not be altered; the taste will be slightly bitter, and leave a slight astringency in the throat. The salt itself ought at first to be employed in small doses, such as the eighth of a grain. Six drachms, divided in doses, of the syrup of morphine, prepared as above, M. Legras says, has cured a diarrhœa which had continued many months. The salt employed to the extent of a grain a day, has never been observed to produce any symptoms of narcotism, though often employed in the dose of two drachms.—*Cercle Medicale.*

Mr. Henry Earle on the Pathology of the Spine.—Dissections have proved to Mr. Earle, that the most distressing nervous symptoms, and even complete paraplegia, may be produced, by adhesions taking place between the membranes of the spinal marrow, (whose freedom of motion is essential to health) and by effusion into the canal, or theca. Affections of the dorsal vertebræ, he found to be much more serious, and more early manifested with respect to spinal disease, than of the cervical or lumbar, chiefly, as it appears, from the difference in the capacity of the canal.—*Phil. Trans.*

Dr. Ranken on the Epidemic Cholera of India.—This is the disease of which Dr. Tytler has published a nosological description, under the name of *Morbus Oryzeus*, or the Rice disease. The symptoms very much resemble the effects of poisons, such as depression and atony of the general system, and accumulation of blood in the central trunks, producing irritation and derangement of the organs of digestion.—*Edinburgh Journal.*

Dr. John Davy on Adhesions and Coagulable Lymph.—On injecting brandy between the lungs and pleura of a dog, firm and long adhesions were formed in twenty-four hours. This is against the received doctrine of adhesions being weak in proportion to

their recentness. When blood is drawn its albumen is liquid, but soon becomes viscid, and at length solid. When in its viscid state, it may be drawn out into transparent fibres and bands which soon become opaque and solid, and attain their maximum of strength in a few hours—This furnishes a key to the natural history of adhesions. Dr. Baillie has committed a mistake on this point in his Morbid Anatomy, Chap. i.—*Phil. Trans.*

Dr. Craigie on the Morbid Influence of the Heart on the Brain.—The celebrated Malpighi, it is well known, was affected with a complicated disease of the heart and of the brain, and several other cases of a similar kind, particulary one by Heberden (*Trans. Coll. Phys. Lond. V.*) are on record, but the close connexion of such diseases seems not to have been much attended to. Dr. Craigie, having met with several cases of this kind, has concluded, that ossification and other organic diseases of the heart, from impeding and retarding the return of the venous blood, tend to produce cerebral extravasation, causing paralysis, apoplexy, coma, and death. He at the same time allows, that cerebral disease may arise "*spontaneously*," or, in other words, that he was ignorant of the causes. Dr. Craigie very justly censures those who, on the dissectiou of an apoplectic subject, content themselves with opening the cranium, and never dream of examining the heart and viscera.—*Edinburgh Journal.*

Dr. Heyting's Case of Chronic Inflammation of the Pancreas.—A woman, aged thirty, pale, thin, and subject to profuse perspiration, applied to Dr. Heyting in May, 1820. She had been safely delivered two months before of a healthy child; but had afterwards been seized with a violent fever, which still continued. She was affected with a profuse and disagreeable salivation. Her mouth and throat were natural, so that the cause of this salivation was not in the salivary glands; but as there was occasionally found in her stools matter similar to that of the salivation, it was thought it might come from the primæ viæ. She had immoderate thirst, but no liquid would remain on the stomach. The perspiration was so abundant, that the bed-clothes had to be changed five or six times a-day. She had regular evening exacerbations of the fever. There was also acute pain of the left side, with dry cough, palpitations of the heart, and a feeling of oppression and uneasiness about the stomach. The abdomen was soft, though considerably tumified. The urine was scanty, with brick-dust depositions. Dr. Heyting decided from those symptoms, that the source of the disease lay in the pancreas, and prescribed the following draught:—

℞ Acidi muriatici oxyg. . . . . ℥j;
Mucil. Gum. Arab. . . . . . ℥ij;
Aquæ Canellæ sine Vino. . . ℥v.
Tinct. Thebaic. . . . . ℈ss.
Sacchari Albi q. s. M.
Capiat cochleare unum quaque hora.

This prescription was successful; for in eight days the salivation, the diarrhœa, and the perspiration abated. The dose was gradually augmented, and followed by tonics, and the cure was completed.—*Hufeland's Journal der praktischen Heilkunde.*

MM. Ribes and Dupre on Sulphate of Quinine in Neuralgia.—In Neuralgia, complicated for the most part with intermittents of several varieties, MM. Ribes and Dupre appear to have been successful in the exhibition of this new medicine, the sulphate of quinine. One of the formulæ employed by Dupre, was the following:

℞ Sulphatis Quininæ gr. viij;
Syrupi Rhei.
Aquæ floris Aurantii . . . . ā ā ℥ij ʒiv.
Ætheris Sulphurici gutt. x.
M.

The Patient took three doses a day, and the sulphate of quinine was increased each consecutive day.—*Magendie, Journ. de Physiologie.*

M. Schlegel on Moxa in Phthisis.—We are quite convinced that moxa, or in other words the actual cautery, has no specific powers of counter-irritation which may not be obtained by blistering; yet as it gives variety to the usual routine in common cases, it may perhaps be tried when patients will submit. M. Schlegel has successfully applied the moxa in phthisis, and particularly in a female, aged thirty-two, in the last stage of the disease, who was cured in two months. We must confess that we are rather sceptical as to this case.—As a substitute for moxa, since substitutes are so much the rage, why might not Dr. Chisholm's boiling water blister be tried?—*Hufeland's Journ. der praktischen Heilkunde.*

Dr. Gimelle on Iodine in Leucorrhœa.—Dr. Gimelle has used Iodine with much advantage in the cure of leucorrhœa of long standing, proceeding as it does from original irritation degenerated into a state of chronic disease. He employed a syrup of iodine in the dose of an ounce night and morning, and friction

every evening with a drachm of iodurated ointment. In this practice attention must be had to the peculiar effects this medicine sometimes produces on the stomach, which are best obviated by the use of acid drinks. The composition of these preparations of iodine is not mentioned.—*Omodei, Annali.*

Mr. Edwards' Case of Poisoning by Arsenic successfully treated with Magnesia.—This, so far as we know, is the third case of poisoning by arsenic, which has been successfully treated with magnesia, as first proposed by Mr. Hume of Longacre, the discoverer of the tests for arsenic, by ammoniated nitrate of silver and ammoniated sulphate of copper. (see also No. 47). We mention this the rather, that we observe Dr. Paris in the new edition of his Pharmacologia, has expressly said, that "alkaline solutions and magnesia, accelerate its fatal effects by promoting its solution." He afterwards, indeed, qualifies this decision in a note, but speaks of it so doubtingly, that few, we think, would prescribe it on his recommendation.—*Med. and Physical Journ.*

Dr. John Hume on Tartar Emetic Ointment and Tonics in Phthisis.—I am desirous of adding to my Remarks on Phthisis, published in the Quarterly Journal for January last, a short sketch of a case, most probably of incipient tubercular phthisis, which at present is in a fair way of terminating favourably.

G. R. a man in his 29th year, by trade a weaver, was, in April 1822, attacked with hæmoptysis. He had some days previously, felt himself unfit for his work, he was weak and languid, and lost his appetite. He had a sense of oppression and weight over his chest, and a severe cough as if he had caught cold, and some pain near the lower end of the sternum. When the hæmoptysis appeared, he went to a surgeon in his neighbourhood, who took a plateful of blood from his arm, and desired him to return in a few days and lose as much more if the hæmoptysis continued. In the meantime, a lady desired him to consult me. When I first saw him on the 25th of April, besides the above symptoms, he had feverish paroxysms occurring daily, for the most part after mid-day; his pulse was 100 and of moderate strength, his tongue rather redder than natural, and his bowels costive. His cough was very severe, and his expectoration frequently bloody, but sometimes of a purulent appearance, I ordered a large blister to be applied to his sternum, gave him a mixture which contained laudanum, digitalis, and tartrate of antimony, and to obviate costiveness the compound rhubarb pill. By the 1st of May the hæmoptysis had disappeared, but the cough was still severe, and the fever had not abated. Bowels were still costive. I now put him on the use of the tartar eme-

tic ointment, one drachm to an ounce of lard, to be rubbed over his whole chest, and the following mixture for his cough:

℞ Mel. . . . . ℥ij.;
Theriac. . . . . ℥ii;
Acet. Distill. . . . . ℥iiij;
Tinct. Opii. . . . . ʒi;
Aquæ. . . . . ℥xii;
M.

Of this he was to take a table spoonful occasionally, according to the violence of the cough. I recommended also exercise in the open air, particularly the swing; and I put him upon a milk diet and animal soup, which he was enabled to procure, by the goodness of a family in whose neighbourhood he resided. Under this management he continued for a great part of the summer, with a fair promise of recovery; to which the antimonial friction certainly contributed, as he used it to a very great extent. In August last, however, from accidental exposure to cold, he had a recurrence of his hæmoptysis slightly, and for two days after its appearance he spit little round substances like pin heads, of a dull white colour, and in considerable quantity. I did not see him at that time, but he got the same pectoral mixture as formerly, and persevered with the antimonial ointment. This man has got through a very severe winter wonderfully well, his house being situated in a sheltered valley, and at present, he is almost free from complaint. His father and uncle, I am informed, died of consumption. He himself is of a fair complexion, naturally well coloured, and in short has somewhat of a strenuous appearance. His recovery must be attributed, in a great measure, to the ointment; for certainly never mortal persevered with such ardour in the use of any remedy. His diet and exercise were tonic, but nothing else. It is intended that, in future, he shall abandon his sedentary employment, and betake himself to one that shall keep him on his legs, and more in the open air. He still uses the ointment.—I may here observe that my approval of Dr. Stuart's practice, is occasioned by conviction, and not by the partiality of friendship; for I have met him once only, since the year 1797, at which time we were both attending the Medical Society of Edinburgh.

I have had a case this winter, of a very severe cough, and particularly expectoration, with little or no fever, which terminated favourably, in consequence of vomiting being brought on, by the patient having taken too much of a compound squill mixture. The vomiting continued for some days; and was not relieved till the patient had his chest and epigastric region rubbed repeatedly with laudanum and camphorated spirit, and

took a pill containing three grains of calomel and one third of a grain of opium every four hours, till the bowels were freely evacuated. This medicine was given in consequence of green bilious matter being vomited. The vomiting never returned, and the cough soon abated; the patient's appetite and strength very speedily returning. Previous to the vomiting, the case had a very unfavourable aspect, as the constitution, without doubt, was disposed to phthisis. To prevent misconception, I wish it to be understood, as my opinion, that phthisis, meaning, of course, scrofulous or tuberculous phthisis, may be cured by tonic remedies, if the disease be in an incipient state, and if proper precautions be employed at the same time; but that, if the disease be far advanced, no treatment whatever will cure it. —*Quarterly Journal of Foreign Med. and Surg.*

M. Magendie on the Action of Pure Emetin.—The action of pure emetin differs from that of the coloured only in being much more energetic. Two grains are quite sufficient to destroy a large sized dog. I have seen vomiting produced, by one sixteenth of a grain, in a man of eighty-five years of age, who, it is true, vomited generally with extreme facility.

I have for some time made use of a pill composed as follows:

℞ Sugar, ʒiv.
Pure emetin, gr. viii. to be made into pills of nine grains.

To produce vomiting, one grain of the pure emetin, dissolved in a little acetic or sulphuric acid, may be administered in water.

The following formula may be employed:

℞ Infusion of linden flowers, ... ℥iii.
Pure emetin dissolved in sufficiency of nitric acid, gr. i.
Syrup of marsh mallow, ... ℥ i.

A spoonful at a dose should be given every quarter of an hour until vomiting is produced.

A syrup may be made after the following manner:

℞ Simple syrup, ... ℔ i.
Pure emetin, ... gr. iv.

Magendie, Formulaire.

Dr. Copland on "Cerebral Excitement."—The current of opinion has lately set strongly in favour of the supposition—for it was no more than supposition,—that, wherever symptoms appeared which might be referred to cerebral excitement, there

must consequently be inflammation, general plethora, or local determination. That the last named state of the circulation may be often present under such circumstances, we will readily allow; but that either of the other two conditions should exist, or be necessary to the production of the manifestations in question, is perfectly gratuitous, and what we positively deny. In support of this we can refer to facts derived from experiment and observation. Bleed a man, or any other animal, frequently, largely, but gradually, either when in good health, or when suffering under some disorder not connected with cerebral excitement: as a consequence of such conduct, if the depletion be carried too far, we shall have symptoms denoting determination to the brain; if farther depletion be instituted, delirium will generally supervene; and even if depletion be carried so far as to produce death, the cerebral derangement will be manifest to the last moment of existence: on dissection, while all the other textures shall be found entirely deprived of blood, the brain will generally evince more that natural vascularity, and always an infinitely greater fulness of blood, relatively, than any other part of the body. We will allow that those effects are not observed if very large quantities of blood are lost, so as to deprive the animal of life in a very short space of time; but here the reason is obvious—the animal dies before the vascular system is accommodated to the mass of blood circulating in it. Now, we assert that we have observed those phenomena which we have described, and have seen those appearances in individuals whose life we consider to have been lost by ultra-depletion; and we farther know that the same phenomena have been uniformly noticed in experiments on the lower animals. But we shall be excused if we briefly illustrate this important point by more familiar examples. How often is it observed in profuse uterine hæmorrhage, that when the patient is but just saved from the immediate loss of blood, great care is requisite to save her from the nervous derangement which uniformly supervenes? Irritative fever is always the consequence, and is more immediately the consequence of the local determination and irritation to which the brain is subjected, notwithstanding that the state of the parts concerned in the process which she had previously experienced might be supposed to divert irritation from that organ. In such cases the arteries running to the head beat violently; sensation is quick and lively; the least irritation of the organs of sense, or excitement of moral affections, is apt to induce delirious manifestations; the lower extremities are pale, shrunk, and cold, while the head is hot and painful, &c. Now, we all know the treatment which alone succeeds in those cases, which treatment farther illustrates that peculiar state of the vascular system, and

of the blood itself, in which the cerebral excitement originates. But not only is local determination, and especially to the brain, the consequence of depletion; it still more familiarly supervenes to a low state of the vital energies of the system: the individual, in whom those euergies are perfect, seldom is subject to those disorders which depend upon local plethora or excitement: it is principally those, in whom the vital or nervous powers of the constitution are greatly weakened, who experience local determinations, or those derangements of the circulation in the brain which are evinced by corporeal and mental derangements. It is chiefly to those individuals that the dictum, "ubi irritatio ibi fluxus," is strictly applicable; and whether the irritation be of a physical or moral nature, the effects will be apparent, and commensurate with its intensity, or with that disposition of the system to which we have alluded.

While we thus contend against certain doctrines which have been lately carried to a hurtful and unscientific extent, let us not fall into the opposite extreme, but let us seek after an intimate acquaintance with the operations and laws of nature, and make them our guide: under such direction we shall shun the more prominent difficulties which surround the exercise of our profession, and in which we shall inevitably become entangled as soon as we lose sight of such guidance, and generalize our pathology and practice beyond the data which a sound observation and experience of the manifestations of nature authorize.—*Lond. Med. Repository.*

Dr. John Hume on Cotton Down applied to Scalds.—I am not aware that it is generally known that cotton down has been used successfully as an application to burns and scalds. It is only a few days since I saw an instance of it, for the first time. A boy, seven years old, had several gallons of boiling water thrown over him, by which he was scaled from the back of his head down to the sacrum, and over the whole of his breast and right arm. In taking off his clothes, all the cuticle was separated from the skin, and the surface left raw. This was immediately bedded with cotton down, and the boy laid on his back in bed, where he lay in a state of insensibility for some days. Whenever matter began to appear through the cotton, it was removed by soaking it with warm water or hog's lard, and fresh cotton was applied. I saw him by chance seven weeks after the accident, when his arms and a great part of his body were healed; and wherever this had happened, the skin was of its natural colour and consistence, and had not the slightest appearance of having been ulcerated. How long this practice has been used here, I do not know. I thought it worth mentioning, as there

used to be a prejudice against cotton as an application to sores.—*Quar. Jour. of For. Med. and Surg.**

M. Player on the Morbid Effects of the Spinal Nerves.—In almost every disease of the extremities, neck and trunk, with its organs and viscera, morbid tenderness may be discovered by pressure or by heat between the vertebræ or in the spinal branches of the nerves which go to the parts affected. Diseases of the head and its organs, appear to depend in part on the same cause, if we may judge from the effect of remedies. This tenderness often exists also on the left of the spine beneath the scapula, and opposite the upper part of the stomach, where the intercostal nerves take their origin. In female diseases, the origin of the sacral nerves are often tender.

The best remedies are bleeding by cupping with large glasses over the spine, and afterwards blistering the part, means which are very powerful in easing pains in gout, rheumatism, phthisis, and cancer. In gout, the fit may thus be certainly cured, taking care always to relieve the stomachic intercostals as well as the nerves going to the toe or hand.—Brande's *Journal.*

CHEMISTRY AND PHARMACY.

On the Electricity produced by pressure.—A very important paper, on the development of electricity by pressure, and the laws of that development, by M. Becquerel, is to be found in the *Annales de Chimie*, xxxii. 5. We cannot do more at present than translate the summary given at the conclusion of the paper.

It is seen, then, that all bodies assume two different electric states by pressure: that, in two bodies being perfect conductors, this state of equilibrium ceases, at the moment the pressure is removed, but if one be a bad conductor, the effect of the pressure continues for a longer or shorter time: that the pressure alone maintains the equilibrium of the two fluids, placed on each of the surfaces; for if the pressure be diminished, and, at the end of a certain time, the bodies be removed from the compression, they will be found to have the electricity, due only to the last or remaining pressure: that heat modifies the development of electricity in a particular manner: that the intensity of the electricity increases, at first, directly as the pressure; and that it is probable this proportion diminishes at high pressures, as the bodies lose their power of being compressed:

* This has been a popular practice in this country from our earliest recollection, and in scalds, where the skin is whole, proves very successful.—Editor.

finally, it is rendered probable, that the light which is disengaged in powerful concussions, is due to the rapid recombination of the two electric fluids developed on the surfaces at the moment of compression.—*Journal of Science.*

Dr. Prout on the Chemical changes of the Chick in Ovo.—This accurate chemist has proved or rendered probable by numerous experiments, that in the earlier stages of incubation, an interchange takes place between an oily matter in the yolk, and a portion of the albumen; which portion of albumen becomes somewhat like the curd of milk, while a portion of the watery and saline parts of the albumen mixing with the yolk, augment its bulk. As incubation proceeds, these watery and saline parts again separate from the yolk, and diminish its volume. In the last week of the process, the phosphorus of the yolk is found in the chick, converted into phosphoric acid, united with lime, constituting the bony skeleton; which lime does not exist originally in the recent egg, but is derived from some unknown source, during incubation.—*Phil. Trans.*

MM. Lecanu and Serbat on Succinic Acid in the Turpentines.—In extracting by heat, the essential oil of turpentine from resin, obtained from the forest of Fontainbleau, MM. Lecanu, Fills, and Serbat, remarked the formation of acicular crystals, which they ascertained to be succinic acid. MM. Henri, Montillard, and Parra, have repeated these experiments uniformly with the same result.—*Annales de Chimie.*

M. Robiquet on the Oil of Bitter Almonds.—It has been long known that the oil of bitter almonds rapidly crystallizes when exposed to the air; and as M. Vogel discovered, the crystals have not the peculiar odour of the oil. M. Robiquet has proved that these crystals possess none of the properties of the volatile matter which escapes, and that they are of the nature of an acid, as they redden turnsole paper and unite with alkalies. They possess none of the poisonous properties of the volatile oil, as was proved by M. Villermé, who assisted in the experiment.—*Annales de Chimie.*

Mr. Brander on coagulation of Salep by Magnesia.—Twenty grains of salep dissolved in four ounces of water, upon thirty grains of magnesia being added, will become in a few hours solid and jelly-like, and will keep for weeks without becoming putrid. The jelly is insoluble in water, fat oils, oil of turpentine, alcohol, or caustic potash. It is partly dissolved by acids, leaving a bulky opalescent residue. The magnesia does not affect

albumen, jelly, or starch in the same way. Nor is the same effect produced on the salep by lime.—*Annals of Philosophy.*

M. Durozier's New Method of preparing Nitric Ether.—M. Durozier placed a tubulated retort, containing about six pints, in a sand bath; the neck entered directly into a serpentine tube, to the other end of which was adapted a receiver, placed in a vessel fitted to keep it cool. At the superior part of the receiver was a safety-tube, communicating with a flask containing a little alcohol, for the purpose of absorbing any ether which might escape. The apparatus being thus arranged, he took three pounds of alcohol at 36°, and mixed with it one pound eight ounces of nitric acid at 32°. The whole was introduced into the retort, and immediately after he poured twelve ounces of concentrated sulphuric acid upon it: the tube was adapted and secured with lute. Five minutes after the introduction of the sulphuric acid, ebullition became manifest; streaks of ether marked the sides of the retort; and soon after it flowed abundantly from the inferior extremity of the tube. When the ebullition had ceased, he removed the contents of the receiver, and found it to weigh twenty-three ounces; he then agitated it with an equal quantity of water, and after standing for a moment, the ether floated pure and limpid at the top. When separated, it weighed ten ounces three drachms.—*Journal de Pharmacie.*

MIDWIFERY.

M. Richter of Moscow's Case of Pregnancy, with Prolapsus Uteri.—A woman, who had long laboured under a complete prolapsus uteri, it hanging down between the thighs, except when she lay in a recumbent position—became pregnant. The pendent uterus was observed to enlarge with the growth of the fœtus, but when she was gone about half her time, it retracted itself, and ascended upwards into the pelvis, so that about the seventh month not a vestige of the prolapsus was visible. On delivery, M. Richter found the orifice of the womb towards the left; the head entered the pelvis obliquely, and forcibly drove forth the left and the anterior part of the lower segment, obstructing the passage of the urine. He made her assume a lateral posture, in order to obviate the oblique position of the uterus, and drew off the urine by a catheter, keeping back, during each pain, the parts which had been forced down. In this manner, he succeeded in effecting delivery. The prolapsus again appeared as soon as the patient was able to be out of bed.—*Petersburger Vermischite Abhandlungen.*

M. Malatides on Congenital Diseases.—M. Malatides, in his "Tractatus de Otalgia," thinks that congenital diseases may often arise from external injury, such as pressure, blows, and oftener still from imprudent venery; in proof of which, he gives a case of an infant, who had been struck on the head from such a cause, "Vir valide in gravidæ ventrem impegit," and when born, the part was livid, and the child had congenital cophosis. —*See also*, Reidlin. *Curar. Med.* 553.

BOTANY.

M. Theodore de Saussure on the Oxygen absorbed, and the Heat evolved by Flowers.—By a number of observations and experiments, M. Saussure has discovered some very curious facts connected with the blossoming of plants. Flowers, he found to consume a much greater quantity of oxygen than the rest of the plant, and in a medium deprived of oxygen, flowers will not blossom at all. Leaves, on the contrary, contain so much oxygen, that when deprived of it they can form a proper atmosphere for themselves. The portions of oxygen absorbed by the flowers, and the leaves, were in *Cheiranthus incanus* 11 and 4; in the double 7.7. and 4; in the *Polyanthes tuberosa* 9 and 3; in the double 7.4 and 3; in the *Passiflora serratifolia* 18.5 and 5.25; in *Cucurbita melo-pepo* (Male flowers) 12 and 6.7; (Female flowers) 3.5 and 6.7; in the double flowers *Tropoleum majus*, 7.25 and 8.3; in the single 8.5 and 3. It follows that simple flowers of equal volume, consume more oxygen than double flowers of the same kind; it is to be remarked also, that simple flowers fade much sooner than double ones. It appears also that, it is at the moment of fecundation, that the greatest quantity of oxygen is consumed; and that the stamina adhering at their base, and to the receptacle, consume more than the other parts of the same flower.

The heat evolved in the spathæ of several species of *Arum* was first discovered by Lamarck, and confirmed by Sennebier (*Phys. Veg. III*). Hubert found that five spadices of *Arum cordifolium* gave 44° R. when the air was only 19° R. (*Journ. de Phys.* 59. *p.* 280). Saussure found that this phenomenon is not confined to the Arums, though it is in them most remarkable. He is inclined to account for it in part, by the rapid destruction of oxygen, or its combination with vegetable carbon at the time of fecundation, which in some of the Arums, he found to be more than thirty times their own volume of gas in twenty-four hours; and in other flowers, their heat was found, in most cases, to be in proportion to the quantity of gas destroyed. On the contrary, some flowers are colder than the air, in consequence of their evaporation; and sometimes, in warm flowers

a fallacy arises from their moisture adhering to the bulb of the thermoscope, and thence evaporating. That the absorption of oxygen, however, is not the only cause of heat, appears from the Bignonia radicans, which, though a warm flower, consumes less oxygen than the *Passiflora serratifolia*, which is a cold flower.—*Mem. Soc. Phys. et Hist. Nat. Geneva.*

Mr. Lindley on the effect of Climate on Rosa Canina.—It is well known that dogs almost lose their hair in some tropical countries, while in the colder regions of the north, the usual hair is thickened at the roots by a close shag or wool. A similar fact is observed in the the common Dog-Rose *(Rosa Canina)* the setæ or bristles of which are, in some northern countries, abundant, while in Egypt and Madeira they wholly disappear.—*Monograph of the Roses.*

AMERICAN INTELLIGENCE.

Experiments on Absorption from the Brain. By the late Dr. J. O'B. Lawrance, and by Dr. B. H. Coates.

(Communicated by the latter.)

My late lamented friend, Dr. Lawrance, and myself, had intended to commence some fresh course of physiological inquiries, but we were prevented from selecting any one, by circumstances, principally consisting in his engagements and occupation of mind, and terminating in his death. The space of one year has thus swept off two members who were engaged in the last course which we made public. We, however, found time for making an experiment, which, as it stands unconnected with others, may be made public without further postponement.

That absorption takes place in the brain has often been inferred both from the effects of disease, and from analogy. This has lately been much strengthened by those observations which go to prove that the veins perform this function. The fact has, however, been often denied, and we know of no positive or experimental proof of it. Since learning the advantage afforded in investigations of this nature by the use of prussiate of

potass, we thought it might assist in ascertaining this point—and we accordingly now have direct evidence of absorption from the contents of the pia matter. About a drachm of a saturated solution of prussiate of potass was injected into the brain of a half grown kitten, the operation was followed by immediate apoplexy. In a short time, the salt was discoverable in the serum of the blood of the right side of the heart—as well as in several other parts of the body.

In one experiment the trials were not made for four or five hours, the animal having been long dead—and in this case, some uncertainty was thought to exist—but in a subsequent one, the fluids, &c. were examined in about an hour, and the results as stated above.

It should be borne in mind that the injected liquor penetrated the ventricles, as well as the lacerated cavity it formed in the brain. I believe this cavity is in the same general predicament with the brain, and has been equally denied to perform absorption. Such as the observation is, however, it is here recorded.

We are not in the practice of correcting the mere verbal errors of the press. But in looking over the excellent paper of Dr. Washington on Yellow Fever, in our last number, we have detected some mistakes, which as affecting the sense, we deem it proper to rectify.

Page 318, line 6, for "Dunbar Castle," read "*Dunstar Castle.*"
320, line 5, for "a moisture was perceptible," read "*no moisture was perceptible.*"
line 17, for "effused," read "*affused,*"
Note, for "Dr. Porlee," read "*Dr. Purlee.*"
322, lines 18 and 20, for "four," read "*ten.*"
327, line 3, for "contradictions," read "*contra-indications.*"
328, 2d line from bottom, for "cathartic," read "*catharsis.*"
330, 13th line from bottom, for "have," read "*has.*"
331, line 5, for "cathartics," read "*catharsis.*"
332, lines 19 and 27, for "effusion," read "*affusion.*"
334, 13th line from bottom, for "employ," read "*empty.*"
335, 2d line for "cure," read "*ruse*"
10th line from bottom, for "ingesta," read "*aliment.*"

25. Geographical and statistical map of modern Europe.
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20	1.50	1.62	2.39
25	1.68	1.77	2.65
30	1.82	1.92	2.93
35	2.00	2.13	3.28
40	2.23	2.43	3.72
45	2.56	2.79	4.29
50	3.03	3.34	4.99
55	3.57	3.96	5.85
60	4.29	4.80	7.00
65	5.23	6.09	8.62

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45	7.90
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60	10.75
65	12.45
70	15.00
75	18.80
80	22.75

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3	289.49
4	265.25
5	245.38
6	229.51
7	215.90
8	203.81

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R. M. PATTERSON, President.
EUGENIUS NULTY, Actuary.

November, 1823.

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TO READERS AND CORRESPONDENTS.

1. We wish it to be distinctly understood, that we neither have, nor will receive, any pecuniary compensation as Editor of this Journal. The motives which led us to engage in the enterprize, are announced in our prospectus, and will be found liberal, and wholly disinterested. To this subject attention is now called, with a request, that communications for the work, and all matters of correspondence relative to it, may be addressed to the publishers, Messrs. H. C. Carey and I. Lea, Booksellers, Philadelphia.

2. We are very thankful, for the communications received for this Journal, from Drs. Church, Morgan, Strudwick, Waterhouse, Anthony, Clarke, &c. &c. all which, shall appear in our next number.

3. To the announcement of Professor Coxe's proposed Work, to be found at the close of the present number, we beg to call the attention of our readers. The design is novel, and important, and we know of no one, who will bring to the execution of it, more curious and recondite learning, or greater precision and fidelity of detail.

4. Messrs. H. C. Carey and I. Lea, will publish in the course of a few weeks, "Essays on the Variolous, Vaccine, and Varioloid diseases, by N. Chapman, M. D."

THE

PHILADELPHIA JOURNAL

OF THE

MEDICAL AND PHYSICAL SCIENCES.

Art. I. *A comparative view of the state of Medical Science among the Ancients and Moderns, its revolutions in different periods of the world, and an enumeration of some of the errors which check its progress.* Read before the Medical Society of the City and County of New York. By John Stearns, M. D.

In tracing the progress of medicine from remote antiquity, it has generally been considered as originating with Esculapius, and acquiring its primary systematic form from the plastic hand of Hippocrates. The first has consequently been viewed as the inventor, and the latter the father of physic. The antecedent ages of the world, including a period of nearly four thousand years, have been indiscriminately consigned to gross ignorance. The first men were supposed to be destitute of science, and in a state of barbarism very little superior to the brute creation, and that their capacity for knowledge was gradually supplied by observation and reflection upon the objects of sense—that science thus commenced, has ever since continued in a state of progressive improvement, which shall be perpetuated till its final consummation in the perfectibility of man. In confirmation of this opinion, the origin of language has been explained by referring to the similitude between the sound

and sense of words, which rude men were said to have invented to express their ideas.

Although from the paucity of historical facts in the early ages of the world, the refutation of such opinions may be deemed very unsatisfactory, enough may, however, be deduced from analogy, and from subsequent history, to prove some to be at least questionable, and others perfectly fallacious.

I assume the position, that medicine flourishes in proportion to the cultivation of general science, and a proof of the existence of the latter, in any age and country, will afford satisfactory evidence of a corresponding condition of the former.

The history of man, the unstable nature of all his institutions, and the revolutions they have successively experienced, afford satisfactory evidence that every art and science have been subjected to correspondent vicissitudes of progression and decline. The perfect obscurity which veils the first four thousand years of the world, exhibits no proof of the ignorance of man, or that his attainments in science were not far superior to those of modern times. Who will presume to affirm, that medical science never flourished during this long lapse of primeval time? or that it was first elicited by the modern Hippocrates? A moment's reflection upon the nature of man may aid our research after the truth.

Allowing for the difference in climate, habits, and education, we may consider the mind of man the same in all ages of the world and in all nations of the earth. When man came from the hands of his Creator, the symmetry of his body and the structure of his mind, sustained the image of their Divine Author, and exhibited specimens of the highest human perfection. His frequent conversations with his Maker, and his familiar acquaintance with all animal and vegetable creation, exemplified in the names which he conferred upon each individual species, prove the derivation of his knowledge of language, and of every necessary art and science, from the same unerring source. With such superior intelligence, aided by a clear perception of truth, he was

well qualified to impart correct knowledge to his posterity, which an antediluvian longevity offered ample opportunities to cultivate and improve. With such advantages, who can hesitate to believe that their Newtons and Hippocrates, carried philosophy and medicine to a perfection that surpassed the conception of modern genius! Sacred history attests to the skill of their artificers in metals—and the pillars of Seth, which resisted the destructive efforts of the deluge, now remain imperishable monuments of the strength and durability of their workmanship. That prescience which enabled Noah to predict the destruction of the world, and the wisdom he manifested in the preservation of his family, and of every species of animated nature, afford ample evidence of a mind endowed with superior intelligence. To affirm that this knowledge was derived from divine revelation, is a confirmation of the argument, and of the correctness of the principles already advanced. It is only from this great fountain of truth that the streams of pure science can ever emanate. It was from this that the first men derived their perception of truth, by a species of intuition, and were thus enabled to avoid those speculative theories which involve the moderns in perpetual error. Although destitute of experience, Adam was thus enabled to govern the family of mankind, to impart to his posterity moral and religious truths, the remedy against a spiritual death, and perhaps a regimen and cure for diseases that protracted life to the astonishing length of nine hundred years. Had the life of a Newton or a Franklin been equally prolonged what limits could we prescribe to their attainments in science—and had they possessed the other advantages enjoyed by the antediluvian philosophers, how much more must they have surpassed all modern conceptions? The limited knowledge of the arts and sciences, which survived the deluge in the family of postdiluvian Noah, was probably sacrificed to the more important object of acquiring the means of subsistence, till its revival was exemplified in the erection of the city of Babel, and its magnificent tower. The miraculous confusion of language, designed to defeat an enterprise, which at once

evinced an unrivalled boldness of conception, and a perfection in the arts unknown to the moderns, formed the greatest epoch in science that ever occurred. Perfect ignorance of former improvements must have resulted from the destruction of all interchange of sentiments and communication of ideas, and enveloped in dense obscurity the succeeding ages of the world. Egypt was probably the only country that preserved the least vestige of those arts and sciences, which the descendants of Ham derived from their antediluvian ancestor. Its remote situation from Babel, perhaps, gave it an exemption from the common calamity which pervaded the East, and might have been the means of perpetuating to its inhabitants, that original language which its Divine Author had peculiarly framed for the successful investigation of truth. We consequently find Egypt, in this early age of the world, distinguished for science and its physicians, for professional excellence, and skill in the difficult and tedious process of embalming. The stupendous monuments and magnificent ruins of this celebrated country, bear ample testimony to a perfection in the arts which succeeding ages have never equalled. We are compelled to acknowledge our ignorance of their art of cementing, embalming, and engraving, and that modern geology has derived some of its prominent improvements from Moses' history of the creation. A brief comparison of this history with the system of Werner, will evince how little the moderns are entitled to the merit of originality. Who would have believed, that in the eighteenth century, men so eminent in philosophy, as Burnet, Woodward, Whiston, Leibnitz, Descartes, Demaillet, Kepler, and Buffon, would have promulgated for truth, the following hypotheses?

1st. That the earth was created from the atmosphere of one comet, and deluged by the tail of another.

2d. That the world was an extinguished sun, on which the vapour condensing formed seas.

3d. That man and all terrestrial animals were originally fish, and gradually emerged from the sea, as they assumed their present form.

4th. That the earth and all the planetary system, were struck from the sun by a violent blow from a comet.

5th. That the globe possessed living faculties: the mountains were its respiratory organs, the veins of minerals its abscesses, and the metals its diseases.

Long would men have continued to invent hypotheses equally absurd and ridiculous, had not Saussure and Werner framed a system of geology, founded expressly upon the information derived from Moses. Guided by this as their polar star, they were enabled to discover the primitive, the transition, the secondary, and the superincumbent classes of rocks, each subdivided and designated by characters indicative of the regular arrangement of the diversified strata which compose the exterior crust of the earth, and the beautiful harmony of universal creation. They also found in the transition class, the fossil organic remains of the most imperfect aquatic animals variously intermingled with cryptogamous plants—and in the mere elevated strata of the secondary class, the remains of the highest order of vegetable life, the more perfect inhabitants of the sea, and the various species of land animals successively arranged exactly in the order which Moses gives of their respective creation. By his account of the deluge, Cuvier also was enabled to explain the condition and situation in which he found the fossil remains of certain animals, the vertical position of particular strata of rocks, and their occasional eruption from their appropriate locality in the interior to the surface of the earth. The same great convulsion also accounted for the immense depth in the interior of the earth, in which the entire bodies of the elephant and rhinoceros were found buried. When we reflect how much light has been shed on this branch of science, by this incidental allusion in the sacred history, we have much reason to regret the destruction of the professional publications of that enlightened age. A preservation of these alone would have rendered the march of science progressive, and prevented the endless hypotheses which have been successively invented, suppressed, and revived in every subsequent period of the world. It

might have disclosed a solution of the singular phenomenon, that among all the fossil organic remains of vegetables and other animals, man is the only one that has never been discovered.

I trust the following remarks, which I formerly suggested as explanatory of this extraordinary fact, may not be deemed inappropriate.

"The hypothesis, that the whole of the inhabited antediluvian world now constitutes the bed of the ocean, thus occluding from our view all remains of human beings, is liable to insuperable objections. As we are unable to compute the period of time necessary to complete the process of petrifaction, we may with more truth conclude, that the fossil remains of shell-fish and other aquatic animals already discovered, have been undergoing that change several thousand years before the creation of man—and as he was the last subject of the sixth day's creation, probably four thousand years subsequent to the shell-fish, sufficient time has not elapsed for the complete petrifaction of his remains. The imperfect skeleton found at Guadaloupe is of too ambiguous a nature to affect this explanation. If that were a real human petrifaction, the process may have been accelerated by the peculiar qualities of the surrounding rock, and thence the precursor to others, which time will soon develop—then will the fossil remains of man be abundantly evident—then will the six days' work of the Creator be perfectly converted into their original component materials, and be typically correspondent to the six thousand years of man's labour on earth—and then will probably commence the grand sabbatical year of rest."*

To reconcile the apparent incongruity of this explanation with the literal history of the creation, we have only to consider it as the usual figurative language of the Scriptures, and assign to each day its common indication of one thousand years.

If any among the Hebrews were equally versed with Moses in the arts and sciences of Egypt, this knowledge was

* Vid. Address to the New York State Med. Soc. for February 1819.

probably lost in the wilderness, and in their subsequent devotion to a military life in the occupation and government of the land of Canaan. And it was not till the splendid reign of Solomon, that we find any allusion to the revival of learning. His superior wisdom, his culture of an unrivalled style of architecture, and of the arts in general—his sentimental writings, the allusion to his publications upon medicine and botany, and his splendid wealth, fortify the opinion, that his age was as much distinguished for attainments in science as he was for pre-eminent intellectual endowments. This literary era, probably, continued during the tranquillity and wise policy which distinguished the government of Solomon, until the Jews became involved in destructive wars, which terminated in their subjugation and final dispersion among other nations. This event, however, contributed to disseminate science at the court of Babylon, where Daniel and his associates were so pre-eminently distinguished.

Although we cannot trace the precise course and causes of the decline of learning in Egypt, there is, however, no doubt of its destruction, and subsequent revival in the establishment of the Alexandrian Library in the year 283 before Christ. This distinguished event rendered it a place of general resort for the literati of all countries. It became the theatre of learning for Greece and Rome, produced an Euclid, and furnished Hippocrates and the Arabian physicians with the means of collecting and transmitting to succeeding ages, a portion of the medical science of that enlightened era. Their writings exhibit a simplicity of practice, founded upon correct principles derived from an actual observation of diseased nature. From these ancient records much has been adopted in modern practice, and claimed as novel discoveries of no ordinary merit. Among these may be included, the affusion of cold water in fevers, which was the common practice of Hippocrates, and has ever since been continued among the nations of the East.

The critical days and other phenomena of disease were marked with an accurate simplicity, unknown to the complicated speculations of succeeding theorists.

Although Hippocrates was entitled to much merit in his profession, yet a knowledge of the history of medicine previous to his time, would probably have detracted from his honours, and especially from his title as the father of physic. That enlightened era, one thousand years before his time, which was distinguished for the erection of the stupendous monuments of Egypt, must have been equally eminent for the production of men skilled in the science of medicine, whose writings reached the time of Hippocrates, and probably formed a part of the Alexandrian Library. When Egypt had become a Roman province, this library, which had continued three hundred years, was entirely destroyed, and with it had nearly perished every vestige of human science. When we reflect how small was the number of books before the invention of printing, we may reasonably conclude, that nearly all the works of antiquity were collected in this institution, the loss of which was succeeded by another era of ignorance, from which Egypt has never since recovered. With the removal of the seat of empire to Rome, was transferred the literary reputation of this and every other subjugated province. The mistress of the world, however, disposed to cultivate science, could not long enjoy its undisturbed possession. The barbarians of the north, equally hostile to civilization and to science, succeeded in the destruction of both, and in accelerating that dark period which buried the world in gross ignorance. The revival of learning, at the termination of this age of obscurity, aided by the invention of the art of printing, may be considered as the real infancy of that era of science, which has ever since continued to illumine the world with increasing lustre.

But although the facility of publishing, and the consequent multiplicity of books, may prevent the occurrence of those periods of ignorance which have so often darkened the world, yet medical science has not had that regularly progressive march towards perfection, which this enlightened age would have given us reason to expect. A variety of causes has retarded its progression, and in some instances given it a retrograde course. Among these may be enumerated the following.

1st. The multiplicity of books.

Although a judicious selection of books essentially facilitates the diffusion of knowledge, their indiscriminate excess may check improvement and the cultivation of genius. The indolent rely too much upon the information of others, the mind ceases to reflect, and genius to invent, and while both thus retrograde from inaction, memory is the only quality of the mind that is exercised and improved.

This constitutes a prominent distinction between the ancients and the moderns. The former had more genius, the latter more learning—the former studied nature, the latter books—the former minutely observed the symptoms of disease as they occurred in concourse and succession, the latter copied them from others.

I would not, however, be supposed to intimate, that wise and judicious publications may not essentially aid the investigation of true science. I object only to that indiscriminate rubbish which loads the shelves of our libraries, obscures the mind of the student, mingles error in all his attainments, and produces a perpetual retrogression from the truth.

2d. The too hasty introduction of new systems of physic.

In reviewing the various systems of medicine which have been obtruded upon the medical public, it would afford much gratification to be enabled to state, that the primary object of each author was the more perfect elucidation of truth, and that each had contributed towards the general improvement of medical science. But I regret to observe, that while some light has been shed by impartial inquirers, others have obscured science, by speculations more indicative of a brilliant imagination than of a sound judgment honestly exerted in the cause of truth. Each has had his period of domination, and each in succession has yielded to a more popular author, strongly recommended by an interested reviewer, aided by the charms of novelty and the plausibility of sophistical reasoning, fortified by factitious cares which never occurred in practice. It will afford very little cause for triumph to the advocates for the progressive

advancement of medical science, to prove from actual experience that this progression has always assumed the form of a circle. The truth of this remark is elucidated by a retrospective view of the successive fate, the alternate rejection and revival of the doctrines of morbific matter, and the humoral pathology of the ancients, of the vascular system of Hoffman and Cullen, the excitability of Brown, the sensorial power of Darwin, the unity of disease of Rush, with the ephemeral opinions of a host of others. I trust it will be superfluous to exhibit a minute and tedious detail of facts, to afford additional evidence of the alternate progression and retrogression of medical science. The sudden and total demolition of the humoral pathology, which for ages had been deemed a standard doctrine in medicine—the stigma of ridicule and heresy, which for half a century attached to every advocate of its exploded principles, and the complete establishment of the vascular system upon its ruins, evince how much a popular teacher may revolutionize public opinion, and even impart to his unstable doctrines the imperative authority of a distinguished university. Numerous errors connected with this system, and inculcated from such high authority, have consequently mingled with every doctrine which succeeding theorists have thought proper to project. The sudden extinction of each by the first glare of light, evinces the error of its fundamental principles, and designates the point of departure from the unerring standard of truth. It is with no ordinary satisfaction, that I have witnessed a partial abandonment of these principles, and a revival of the doctrines of Boerhaave in the medical school of New York, where the acute reasoning, sound deductions, and important facts, treasured in the writings of this able professor, and of his learned commentator, are taught to assume their proper station in our libraries, from which they had long been discarded by works infinitely inferior in merit. These vacillations in opinion, and revivals of old systems, have been equally conspicuous in the science of chemistry. The doctrines of Lavousier, which effected such an entire change in the fundamental principles of this science,

were found to have been taught by Mayhew and V. Hook three hundred years before.

3d. An anxious avidity for new remedies. Perhaps no error has been productive of more injury, than a solicitude to supplant old and approved medicines, with such as the young and the ambitious are disposed to obtrude upon the profession. Hundreds of lives may be sacrificed, before the deleterious qualities of a new remedy may be perfectly ascertained, or its precise operation determined. Such have been the effects of the digitalis purpurea, and such will continue to be the effects of the Prussic acid till it is discarded from the Materia Medica. The long list of similar articles, whose ephemeral celebrity has yielded to a knowledge of their fatal effects, is too familiar to every practitioner to require a particular enumeration. The modern doctrine of diseased solids, has induced the general adoption of those Sampsons of the Materia Medica which the ancients used with the greatest caution. By an indiscriminate use of these, has been introduced an energy of practice designed to terminate the disease, without reference to those indications of nature which were the favourite guides of former practitioners. A partial dereliction of cutaneous applications, and a general direction of remedies to the stomach, have been the effect of this change. When we reflect how extensive a surface the skin presents for the operation of remedies, and how intimate is its sympathy with almost every organ in the body—and how efficacious and useful have been the remedies when applied to its surface—we cannot but regret the prevalence of a doctrine that should have discarded so useful a portion of practice. Diseases of the liver, of the intestines, stomach, and uterus, are well known to be indicated by appropriate affections of the skin, and if investigated with a suitable reference to this sympathy, might disclose remedies perfectly adapted to the cure of each, by their simple application to that portion of the surface to which the symptomatic affection is confined. Since the discovery of vaccination, the attention of the profession has been more

particularly directed to this subject. But in all the recent publications of Jenner, and others, upon the external and internal use of antimony, in its various forms, as if the result of a late discovery, we look in vain for any thing new, or that was not better understood by the physicians of the last century. Frictions, which they considered so essentially conducive to health, and to the removal of many diseases, have been comparatively disregarded by the moderns. For the prevention and cure of many diseases connected with the skin, the Materia Medica does not present a more efficient remedy, when used agreeably to the directions and restrictions of former practitioners.

4th. The fashionable practice of simplifying systems and remedies.

The living body is a complicated machine, composed of an infinite variety of parts, mutually connected and mutually dependent, and the whole participating in the pains, diseases, and lesions of each. Any system of medicine, that is not founded on this complex structure of parts, and has not an immediate relation to all its minute ramifications, however plausible in theory, must be manifestly erroneous in its fundamental principles, and productive of much injury in practice.

Thus, the humoral pathologists erred in exclusively limiting diseases to the fluids, in attempting to correct which, Cullen erred in the opposite extreme. Brown's scale of diseases, founded upon the excitability of the system, and the unity of disease as taught by Dr. Rush, exhibit a refinement in simplifying, which required all the genius of the former, and the fascinating eloquence of the latter, to have given to either an ephemeral popularity. "*Delenda est nosologia*," was the favourite maxim which so essentially conduced to give success to their opinions. The same principle has led authors to identify, as one and the same disease, those of a perfectly distinct and opposite character. To denominate the yellow fever a high degree of bilious remittent, is one of the many exemplifications of this position that might be adduced—

and which I shall further elucidate, by a brief exhibition of their distinctive and discordant natures, and thus prove their combination into the same disease to be erroneous, unnatural, and violent.

Diagnostic symptoms which distinguish the Bilious from the Yellow Fever.

1st. The pulse in bilious fever is tense, full, and generally regular, and is always accelerated as the disease approaches to a fatal termination.

In yellow fever the pulse is soft, irregular, sometimes natural, and is always more or less retarded as the disease approaches to a fatal termination.

2d. Except the usual flush of ordinary fever, the bilious exhibits no unnatural change in the countenance, or in the appearance of the eyes.

In yellow fever, the eyes exhibit a peculiar combination of red and yellow suffused over their whole surface, and the whole countenance has a wild and ferocious aspect, which is strongly indicative of the disease, and which I have always considered an infallible criterion of its true distinctive character.

3d. In bilious fever, the skin is hot, tense, and elastic.

In yellow fever, the skin is soft, flabby, and generally colder than natural.

4th. In bilious fever, the debility of the muscular system always increases, as the disease approaches to a fatal termination, and the patient is often unable to move, while the arterial action continues stronger than natural.

In yellow fever, the strength of the muscular system is always in an inverse ratio to that of the arterial—and after the pulse has ceased to beat, and the extremities have assumed the coldness of death, the patient is able not only to rise from his bed, but to walk the room, and often to express his conviction that he is better, and will soon be well.

5th. In bilious fever, the intestines are excited by ordinary cathartics.

In yellow fever, the same doses have little or no effect, and the costiveness is often extremely obstinate.

6th. In bilious fever, the coffee-ground vomiting is a very rare occurrence.

In yellow fever, a case seldom terminates fatally, without exhibiting this species of vomiting, which is always characterised by a convulsive effort of the stomach, without exciting a correspondent action of the abdominal muscles, so common in ordinary vomiting.

7th. In bilious fever, the yellow aspect of the skin which sometimes occurs, is essentially different from that peculiar appearance which generally takes place in yellow fever. In the latter it nearly resembles the colour consequent on a bruise.

8th. In bilious fever, respiration is generally accelerated, but regular.

In yellow fever it is slow, irregular, and often interrupted with sighing.

9th. In bilious fever, the symptoms and dissections evince a redundancy of bile.

In yellow fever they exhibit a deficiency of bile.

10th. Bilious fevers generally continue from nine to twenty-one days.

Yellow fevers seldom exceed the eleventh, but generally terminate fatally before the seventh, and often on the fifth day.

11th. In bilious fever, the tongue is invariably covered with a coat of fur, which, as the disease advances, becomes thick, dark, and sometimes black.

In yellow fever, the tongue is generally free from fur, and as the disease advances, it becomes still more clean, and often entirely natural in its appearance.

12th. Bilious fever often commences with an intermittent type, changes to a remittent, and then to a typhus or continued, but is never converted into a yellow fever, or what has been erroneously denominated *typhus icterodes*, without being exposed to a cause which produces this disease *ab initio*.

Each runs its distinct course, without a mutual conversion of symptoms or confusion of character, equally in the mild as in the malignant stage, from the commencement to the termination of the disease.

13th. Bilious fevers prevail in all places, sections and latitudes, equally in the country as in the dense population of cities.

Yellow fevers generally originate in the tropical climates, and when they prevail in the temperate latitudes, they are always restricted to commercial places, or to a population in the vicinity of the tide waters of the ocean, but are never known to extend to the cold regions above the 45th degree of latitude.

14th. Bilious fevers prevail at all seasons of the year.—Yellow fevers invariable cease immediately after the frost and cold have become regular and severe.

This evidence might be easily pursued to a much greater extent, but enough, I trust, has already been adduced to prove an essential difference between bilious and yellow fever, and to evince the error of the prevalent opinion, that the latter is only an aggravated degree of the former. The errors that have occurred in deciding upon the true diagnosis in the incipient stage of the disease, have undoubtedly originated more from a want of a correct and judicious observation of the diagnostic symptoms, than from any defect in the characteristic distinction.

The specific difference between small pox and measles, and between many other diseases whose alliance has never been suspected, is much less distinctly marked, than between bilious and yellow fever.

I trust, therefore, that the confusion which this want of precision in discriminating diseases, necessarily introduces into the theory and practice of physic, may be very obviously included among the errors which retard the progress of medical science. And until these errors are entirely excluded from our medical journals, we shall have to lament the want of that precision in the diagnosis of diseases, which essentially conduces to that elevation of character which

the medical profession should always sustain, and to which it is justly entitled by its necessary connexion with the health and lives of mankind.

The mania for simplifying systems of medicine, has introduced an equally erroneous practice of simplifying remedies. If the fixture of the excitability of Brown, to the medium point, were all that is nesessary to constitute health, the exhibition and abstraction of stimuli would be perfectly adequate to remove disease. But when we reflect upon the complex nature of the human system and its diseases, we must be convinced that an equal complication of operations is absolutely necessary to exterminate disease from every point.

But in denouncing this extreme, I would equally avoid the practice of our predecessors, who combined in their famous methridatum and theriaca at least sixty different ingredients.

I trust the time is not remote when closet systems, and closet remedies, will be supplanted by those which are founded upon actual experience and correct observation. Facts derived from nature, as exhibited in disease and in health, should be our only guide, uninfluenced by preconceived theories, or a desire to sustain a favourite party. And I ardently hope, that some Newton will yet appear in our profession, to establish medical science upon the immutable basis of truth, too cogently demonstrated, ever to be shaken by the speculating innovations of succeeding theorists.

ART. II. ***New Division of Apoplexies.*** By M. A. SERRES, Chevalier of the Legion of Honour, one of the Physicians of the Hospital of la Pitié, Chief Director of the Hospitals, &c.—Translated from the original by *George B. Taylor,* Student of Medicine.

> "Antequam de remidiis statuatur, primum constare opportet, quis morbus et qua morbi causa, alioqui mutilis opera, inutile omne consilium." BAILLOU, lib. i. Cons. XIV.

THERE are few medical subjects which have employed observers so much as apoplexies. Galen, Wepfer, Bonetus, Sennertus, Boerhaave, Valsalva, Morgagni, &c. have exhausted in the consideration of these diseases all the sagacity and talent of observation which they could command. This imposing assembly of eminent names would have deterred me from commencing new researches, if, notwithstanding their labours there could not still be found a most deplorable uncertainty as to the seat, diagnosis, and treatment of these affections. Science sincerely feels this defect. Perhaps it belongs only to hospital physicians to remove this uncertainty, because they alone are placed in divers circumstances of a favourable nature to make those observations which are requisite for such an undertaking. It is only in establishments of this kind that the observer can meet with a sufficient number of facts, calculated to remove these doubts—that he can devote himself to a strict observance of the symptoms of these diseases, by studying them night and day, previously, during, and after the administration of the curative means, which often change their order.

It is only in hospitals that the pathologist can arrive at pathological discoveries, according to the happy expression of Valsalva and Morgagni. How many errors has the superficial observation of a corpse given rise to! The pathology of the brain, in particular, owes to such a circumstance the state of infancy in which it still exists. This I shall have occasion to substantiate, in the consideration of apoplexies. The work that I am about to publish, on these dis-

eases, and of which this essay forms but a fragment, is the fruit of seven years' investigation, at the Hôtel Dieu and Hospital of la Pitié, upon about two or three hundred apoplectics. I shall appropriate this memoir to their diagnosis. I shall show, *First*—What has been the cause of the versatility of the proposed divisions, rejected from time to time until the present period. *Second*—I shall examine whether the effused fluids in apoplexies are a cause, or an effect, of these diseases. *Third*—I shall mention the new divisions, to which the consideration of their symptoms has led me, confirmed by the necrotomy* of more than one hundred dead bodies, many of which have been publicly examined before the students of the hospitals, in my anatomical lectures. *Fourth*, and *lastly*—I propose to myself a solution of the following problem: Apoplexy being admitted to exist, to determine its seat by the symptoms.

SECTION I.

Causes of the imperfect diagnosis in Apoplexies.

There is a good division of apoplexies required in science, said Stoll, after Lancisi.† Why is it wanted? This is the first question that I propose to examine. Hippocrates has left nothing positive upon the diagnosis of apoplexies.‡

* Derived from Νεκρος, cadaver ipsum, a corpse, a dead body, and Τομή, section, incision, dissection; is equivalent to the Latin "sectio cadaveris," and literally signifies the dissection of a dead body.

† Nondum adesse videtur, vera divisiæ apoplexiæ. Stoll, Ratio Med. sec. iii. page 361.

‡ They still cite the forty-second aphorism, section second, as containing a division of apoplexies, without noticing that the ancients confounded apoplexy with palsy. It is only since the Commentary of Lindanus that there has been established, in the writings of Hippocrates, a distinction which has rendered the text of many ancient writers unintelligible, particularly that of Hippocrates, of Aretæus, and of Ægineticus. These authors speak of apoplexy of the thigh, (Ægineticus, book iii. chap. 18) of the tongue, arms, hand, &c. (Hippocrates, sec. vii. No. 40, vid. Lorry, Prædict. book ii. chap. 10, *Chartier* vol. viii. Prænotion, No. 559, &c.) who report entirely what we design to convey by the word *palsy*. In the writings of these authors, and often in those of Celsus, the words αποπληξια, ἡμιπληγια, παραπληγια, are synonimous, and are applied indistinctly to apoplexies, he-

Galen, seeking to determine their seat, laid the foundation of the systematic ideas, which have embarrassed the efforts of observers until this moment. He forced a bistoury between the occipital bone and the first or atlas vertebra of a mare. The instrument passed into the fourth ventricle of the brain, the animal fell and died the next instant in an apoplectic state*—an evident proof, adds the Doctor of Pergamus, that apoplexy is the result of the flowing out of the animal spirits, and that these spirits are secreted within the cerebral ventricles.

I cannot sufficiently admire, says Wepfer,† the credulity of anatomists, who have admitted and repeated this explanation. Who, in reality, does not see that, before penetrating into this cavity, the instrument would cut the spinal marrow and vertebral arteries? Is there any occasion to invoke the draining of the animal spirits, and the highest order of ventricles, when, before coming to them, we meet with so many causes of death? Should one expect to see the same author add, after these wise reflections—"it is not in the ventricle, but rather in the medullary substance, that the animal spirits are secreted.‡"

Wepfer only changed the hypothesis of position of Galen and Riolan. Such was moreover the manner of philosophising in his time. A principle has been assumed that the explanations of the phenomena of a disease cannot immediately be explained from observation—such combined sup-

maplegias, and paralysis. By not observing this fact, commentators have attributed to Hippocrates errors, to which he is an entire stranger. Thus they have made him say, that fever cures apoplexies, when all physicians have signalized it as the most dangerous symptom. Establish the text, and in place of *apoplexy* read *palsy*, and Hippocrates will no longer be in opposition to experience. In considering the writings of this great observer, I think I perceive that he uses the word ἀφωνια, when he wishes to distinguish apoplexies which are not accompanied by paralysis. (See, on this subject, Van Swieten, vol. iii. p 252, and following; Burserius, vol. iii. p. 53; Lorry Comm. sec. 7th. on aphorisms.)

* Wepfer, Hist. Apoplect. p. 121.

† Idem.

‡ Idem, 232, and other places.

positions must be recurred to as might emanate from effects analogous to those that they wished to explain. Medical men, it is painful but necessary to say so, have remained but too long faithful to this principle. No sooner was the insufficiency of the animal spirits acknowledged, than they hasened to create a new hypothesis in its stead, which naturally presented itself in the fluids poured out in the cerebral cavities in the course of apoplexies. They established without proofs, that these compressed the encephalon by their weight, and that compression was the mechanical cause of apoplexies.* This supposition once admitted, it becomes easy to divide apoplexies into sanguineous and serous, according as the fluid discharged was of the former or of the latter description. They might even establish a purulent apoplexy in the case, sufficiently frequent, in which suppuration terminates the apoplectic irritation,† upon the same principle that Boerhaave and Haller admitted an urinous apoplexy in a case in which the serosity showed outward characteristics of this fluid.

Be this as it may, the basis of this division cannot be ascertained till after death: of what utility, then, I ask, can it be at the bed of the sick? Is it after death—is it on the corpse, we should found our hopes of cure? It was to refute this objection that Preysinger, Sennertus and Riverius laboured to assign to apoplexies, symptoms which caused them to be known during life.‡ But in speaking of a principle so uncertain as that of compression, it was difficult to arrive at satisfactory conclusions and not to fall into error, which has been a just reproach to the characters which these authors assigned to sanguineous and serous apoplexies.

* Riverius' Practice, chap. 2. (Praxis Med. cap. 2.) I know no medical theory which has thrown more light, and carried with it more general credence, than that of the compression of the brain by effusion of the fluids. Surgery has based upon the indication one of its most bold operations—trepanning.

† Morgagni Epist: Anatomica Medica, V.

‡ See, on this subject, the Researches of professor Portal—1st. Memoirs of the Academy of Science in the year 1782—2d. Memoirs of the Institute, 1803.

It is not even without a painful sentiment that we read the reproaches that this division occasioned—some patients, treated for sanguineous apoplexies, presented on a *post mortem* examination, all the cerebral cavities full of serosity—others, with whom they thought they met with all the signs of a serous apoplexy, presented inflamed sanguineous cavities formed in the thickness of the lobes of the brain, or located in the *corpus striatum*, thalami optici, &c.,* so that one ventircle was full of blood and the other of serosity ;—a purulent collection had succeeded to the irritation of the brain.† In fine, at other times those cavities were empty, and the causes of apoplexy appeared to vanish with death, as said Quarin,‡ after the Doctors of Breslau.||

It is asked, after reading these facts, with which the annals of science are filled,¶ how so uncertain a division could prevail so long? It cannot especially be accounted for by the surprise in finding the illustrious *Morgagni* among its most zealous supporters. There are reported in opposition to it, apoplexies without effusions. Cannot this author reply, that they have forgotten to mention this fact?** Has he himself found similar cases? I request you to observe, adds he, that it is less by the quantity than by the quality of the fluids effused, that I am confounded. Some drops of serosity, slightly saltish, appeared to him sufficient to develop the apoplectic symptoms, convulsions, and death.†† Boerhaave had already said something similar in his Treatise on the Disease of the Nerves ;‡‡ but Lecat§§

* Portal, Observations on the Nature and Treatment of Apoplexy; Bonetus, Sepulchretum, vol. i.

† Epist. Anat. V. Morgagni de Sed. et Caus; Bonetus, Sepulchr.

‡ Chronic Diseases, *Apopl.*

|| Hist Morb Bratisl. Apoplexiæ.

¶ Wepfer, Historia Apoplecticorum; Bonetus, Sepulchretum; Morgagni, Epist. III. IV. V; Brunner, de Apoplexia Ephem. Gerus, Obs. 153; Bang, Praxis Medica, vol. i.

** Epist. IV.

†† Epist. IV. sec. 2, 4; Epist. VIII. No. 1, 5, 7, 27, et alibi.

‡‡ De Morbis Nervorum, vol. i. p. 188; De Acrimonia sanguinis in vasis Pia Matris.

§§ Burserius, Inst. Medicine, vol. iii. p. 82.

and Veikart* adopted, unreservedly, this etiology of apoplexy.

I beg those who now-a-days consider the determination of the seat of diseases useless investigations in medicine, to think on the practice of Vieussens.† They will perceive it to be succeeded by the extinction nearly of these imaginary acrimonies. (*I'ls le verront poursuivre jusqu' à extinction, ces acrimonies imaginaires.*) Neither the complication of accidents, nor the increasing severity of the symptoms, nor death—nothing can shake the confidence which he has in this doctrine.‡

SECTION II.

Of Nervous Apoplexies, Sympathetic, &c.

The genius of two men has exercised upon medicine the greatest influence. It is round that of Boerhaave and Stah that medical men have rallied, since the extinction of Galenism. As long as the system of the professor of Leyden (i. e. Boerhaave) reigned in the schools, the insufficiency of the division of sanguineous and serous apoplexies was uselessly opposed. Varoli, the Doctors of Breslau, Hoffman, Casimic, Medicus, Rahn,§ Tissot,‖ and many other observers, report cases of apoplexy, without any effusion, in vain. We must believe, without reasoning, such is the spirit of the sects in all ages.

All the affections seemed to be mechanical in the system of Boerhaave, all nervous or sympathetic in the theory of Stahlianism which sprang from its ruins.

What is understood by nervous or sympathetic apoplexy? Primarily, an immaterial irritation was supposed to be fixed in the cerebral substance, or upon an organ more or

* See Burserius's Commentary on Brown.

† Vieussens's works, p. 400.

‡ Vieussens on Apoplexy, p. 400, and following—see, especially, eighth observation.

§ De mira capitis et abdominis consensu, p. 78.

‖ Epist. Med. vari, p. 80, 90.

less distant, the irradiations of which acted upon the brain and developed the apoplectic symptoms. Such had been the opinion of nervous apoplexies, of Vanhelmont, of Stahl, of Sydenham,* of Hoffman,† an opinion which was renewed by Tissot,‡ and by Bordeu,§ and several moderns,‖ particularly that of the authors who placed the seat of apoplexy in the stomach,¶ the intestines, the liver, the gall bladder,** the lungs††—the production of which was explained by sympathy of the organ, an expression to which the vague ideas relative to these diseases are attached.‡‡

But it was soon perceived that the encephalon was not so much a stranger to apoplexies as this etiology supposed—and nervous apoplexies, without cause, were created with eagerness, in cases where no kind of effusion was found—and nervous apoplexies, with cause, where the brain was materially changed in its structure.‖‖ Nearly about the same time M. Montain, of Lyons, renewed the division of venous and arterial apoplexies already proposed by Chandler and Cullen,§§ by adding to it two new species, *sthenic nervous apoplexies*, and *asthenic nervous apoplexies*,*** which Brown and Veikart††† had accredited so long a time. In fine, after having combated these last divisions, the au-

* In. diss. Epist. cit. by Baldinger, vol. iii. c. 4, p. 84.

† Med. Syst. part ii. ch. 3, sec. x.

‡ Epist. Med. var.

§ See, on this subject, the works of Deseze, Researches on Sensibility.

‖ See professor Portal, the work cited.

¶ Vanhelmont, Eth. Muller Praxis Med. 1, 2, on Apoplexy.

** Weisbrecht, Comm. Litt. Ann. 1744, Hebd. g. No. 2. F. Hoffmann, Med. Syst. Rat. vol. iv. p. 2, sec. i. chap. 7. Eph. Nat. Cent. iv. Observation 159.

†† Pros. maitium in Comm. Hipp. tex. 2. Fracastorius Epist. carab. Malpighi Lancisi Epist. Med. p. 140.

‡‡ Schœrer, Epist. Th. vol. ii. p. 376. Moll, de Apop. biliosa, Burserius, vol. iii. p. 65.

‖‖ Richelini, Essay on Apoplexy, 1811.

§§ Vol. ii p. 202, 1109.

*** Treatise on Apoplexy, by M. Montain, 1811.

††† See the Commentary already cited.

thor of the article Apoplexy, in the Dictionary of Medical Sciences,* admits eight species of them, by giving to the hypothesis of the compression of the brain from liquid effusion, an extension which the observers had not hitherto known how to support, although they had returned some years to the serous and sanguineous division of apoplexies.† Such is the picture that the history of apoplexies presents—uncertain divisions, an unsettled theory—of misrepresentations and errors. This is what the spirit of system has produced. Is it possible to arrive at notions more positive, in taking for our guide observation and experience? I am going to attempt to determine, in the commencement, whether the effusion in apoplexies is a cause or an effect of these diseases.

SECTION III.

Are effusions the cause or effect of Apoplexies?

We have seen that the division of sanguineous and serous apoplexies, and that proposed latterly by M. Lullius, Winslow, &c. rest entirely upon the idea that the effusions in those diseases within the encephalon, determined by their compression all the symptoms of those affections;—and we have seen that surgery had based upon it one of the most daring and desperate operations—that of trepanning.

This opinion had even sprung from such deep roots, that they were nearly proposing to perforate the cranium of the apoplectics in order to drain off the effused fluids.

It seems that to establish or admit a doctrine of this importance, we must convince ourselves by experience whether these fluids really exercise this action—whether they be cause or effect of apoplexies. It has not been done, and this neglect has been attended with most pernicious consequences in the pathology of the brain.

* Vol. ii. p. 239, 240.

† The Annuary for 1814, and this memoir, having been delivered at that time, I pass over in silence what was subsequently published.

To determine whether the effusion of the fluids may or may not produce the apoplectic symptoms, appears to me of the highest importance in regard to the pathology, still so obscure, of that organ.

In order to give to these researches the degree of certainty of which they are susceptible, I have thought it my duty to proceed in this examination, first upon animals, and afterwards upon man. I am going to submit to the judgment of the profession the results which I have obtained.

FIRST EXPERIMENT.

I trepanned an old dog on the middle part of the cranium, corresponding with the superior longitudinal sinus. I introduced a very small bistoury in such a manner as to open the sinus from one end to the other. I shut up the external opening in order that a sanguineous effusion might take place within. I set the animal free: he ran about the laboratory, endeavouring to escape at the same time. Three hours after, his natural state was so little changed that we doubted whether an effusion had developed itself.

On opening the cranium we found a very considerable clot of blood between the lobes, and a second extended to the left hemisphere.

SECOND EXPERIMENT.

I repeated the preceding experiment upon a young dog, because it might be objected that the sinking down of the brain in the old dog left a void between the cranium and it, which would have rendered the effect of the compression void in the first case. The result was the same—the dog ran about the whole day—he eat and drank as usual. The effusion was situated in the same place, and extended over the two hemispheres.

I did the like with several rabbits—I did so with birds—pidgeons, jackdaws and magpies, and still obtained the same result*— no somnolency, none of the symptoms which

* I was a long time finding this simple method of producing artificial effusions in the cranium, or encephalon. The means hitherto

accompany apoplexies. By the preceding experiments I have reduced the question to its most simple state—I have in nowise touched the cerebral substance—every symptom which was manifested should therefore be attributed to the pressure of the blood poured out. There was no other development. This was already a strong presumption against the generally received opinion: for I could compare this effusion to those which occur in the course of apoplexies, between the dura mater and the arachnoid coat, or between this and the pia mater—apoplexies in which the substance of the brain is not involved.

SECOND SPECIES OF EXPERIMENTS.

Artificial effusions in the Ventricles.

But would there be the same effusions formed within the ventricles? Would not the apoplectic symptoms develop themselves?

Many and new experiments were necessary to determine these questions.

THIRD EXPERIMENT.

I took a grown dog, which was trepanned as the preceding, on the middle part of the superior longitudinal sinus: this being done I introduced a bistoury directly into the great interlobular scissure. I pierced the corpus callosum, and so directed the point of the instrument to the left as to enter the ventricle of that side.*

I withdrew the instrument and closed the external opening. The animal had a vertigo of a minute's duration—it was uncomfortable the whole day—had a little agitation in the pulse, and also violent thirst, but there was no som-

used were very defective—1st. Because they consisted in introducing a foreign fluid, the action of which was more or less irritating—2d. Because the tube of the syringe which was introduced into the cranium, and the effort which it was necessary to make in forcing the liquid to enter, already exercised a very strong compression. By my method, the artificial is entirely similar to the natural effusion.

* I should inform those who might wish to repeat this experiment, that it was not until after several fruitless attempts that I hit upon the ventricle.

nolency: his sleep in the night was troubled. In the morning he walked about in the laboratory and appeared less sick than the old dog. In three hours the cranium was opened: the effused blood had filled the great fissure between the lobes, and penetrated the left ventricle, which contained an ounce and a half of it—a small cavity was found in the anterior part of the corpus callosum (middle lobe.)

FOURTH EXPERIMENT.

A rabbit two months old was subjected to the same experiment—set loose soon after, he ran in the garden five or six minutes without stopping: he was finally taken: I caused the cranium to be opened in order to ascertain whether the effusion had formed. It had in effect occupied the same scissure in the interior of the right ventricle and the basis of the cranium, where it had been opened by a slight destruction of substance made on the anterior part of the corpus callosum.*

THIRD SPECIES OF EXPERIMENTS.

Artificial Excavations formed in the cerebral substance.

In the preceding experiments, the cerebral substance not having been concerned in the part of the corpus callosum, one might believe that the brain had not been submitted to the immediate action of the weight of blood. In order to prevent this objection, to put the animals in the condition in which apoplexies are so frequently found, it was necessary to form artificial excavations in the brain itself and to prove the result.

FIFTH EXPERIMENT.

An old dog was chosen (because of the existence of apoplexies at that period of life in man)—we began by making

* In order to have a result comparatively exact, I have noted—1st. the age of each animal—2d. its total weight—3d. the weight of the head particularly—4th. that of the brain—5th. that of the blood spilt compared with the weight of the encephalon. The post mortem examinations were in this manner made in the human subject.

with the trepan an opening in the lateral and somewhat posterior part of the longitudinal sinus—the bistoury pierced transversely the left hemisphere, and brought away a certain quantity of its substance (ʒij). No somnolency or impeded respiration supervened. An excavation containing a coagulum of the size of a nut was situated at the middle part of this lobe. On another animal I made a cavity in each lobe, with the same result—no apoplectic symptoms. The same result with a pidgeon—the two lobes of which I pierced with a very large pin: a transverse cavity thus made from one side of the cranium to the other produced no somnolency.

SIXTH EXPERIMENT.

I made an artificial opening into the middle part of one of the hemispheres: I took away a certain quantity of the cerebral substance: I thrusted a cork into the aperture in the manner of a plug, so as to augment the compression.—There was a complete hemiplegia, but no apoplexy, no somnolency.

I have repeated these experiments upon rabbits, and on birds: the effect has been the same. I have made them upon oxen and upon horses—and shall give an account in another article of the object which directed me in these researches.

Thus sanguineous effusions do not produce apoplexies, whether they be lodged between the cranium and the dura mater, or between that membrane and the brain—whether they occupy the grand interlobulary fissure, and in this manner rest upon the corpus callosum—whether one has formed a cavity in the hemispheres, before, behind, or in the middle, or pierced it from one side to the other—whether, in fine, in traversing the corpus callosum (middle lobe) they had penetrated into the ventricles and filled these cavities. The same result with birds—the same result upon rabbits—the same result upon dogs. The apoplexy of man therefore cannot be attributed to the presence of the effusion of blood, whatever place it occupies, whether it be

found out of the brain, or in the cavities of that organ, or lodged in its proper substance. Such is the analogical conclusion which these facts warrant.

EXPERIMENTS RELATIVE TO MAN.

These experiments conducted, and their results confirmed two or three times, in presence of the students who attend the course of the amphitheatre of the hospitals and several distinguished surgeons,* it was interesting to seek upon what foundation the contrary opinion was established. Do the facts of pathological anatomy relative to the brain of apoplectic persons contradict my experiments? Has there been observed a constant relation between the development of apoplexies and the formation of effusions? Can the doctrine of compression by effusions account for the formation of apoplexies, their progress and termination by death or recovery? Let us consult on this subject the annals of our science.

FIRST OBSERVATION.

A girl of two years old died in a hydrocephalic state, without having presented either somnolency, convulsions, or paralysis. On the opening of the body, Vesalius† found the cerebral circumvolutions and ventricles distended by an enormous quantity of serosity. He estimated the weight of it at nearly nine pounds.

SECOND OBSERVATION.

Wepfer reports the case of an old man seventy years of age, who died of consumption. He had spoken to the moment of his death, and complained of pains in the thighs. On the opening of the body, there was found in the ventricles and between the meninges a great quantity of limpid serosity.‡ If, in the case reported by Vesalius, it might

* Dr. Lisfranc, one of the surgeons of the central ward, and M. Mortier, chief surgeon of the Hôtel Dieu, of Lyons.

† Vesalius, de Fab. corp. hum. l. i. c. 5.

‡ Hist. Apoplecticorum, p. 229.

be said, that the little resistance of the bones of the cranium had hindered the effused fluid from exercising its compressing action upon the brain, what will be objected to the examination of this old man?

THIRD OBSERVATION.

I assisted at the opening of the body of a young man* who died of an ataxic fever—the delirium lasted to the moment of death. The brain was a little softened: but what above all struck the students, was the enormous quantity of serosity which occupied its infractuosities, the four ventricles and the canal of the spine.

FOURTH OBSERVATION.

Another old man of seventy years of age, died dropsical in the clinical ward of la Charité, the fourth of March 1810. He preserved his senses until his death. On the opening of the body there was found sanguineous serosity in the left ventricle of the brain, and clotted blood in the right.†

No symptoms of apoplexy.

To these citations I limit myself. There are related in the memoirs of the Royal Surgical Academy, (Goursand), in the work of Amatus Lusitanus, in that of Bonetus (Sepulchretum,) and in those of Morgagni, cases, where purulent collections existed in the brain, and which did not produce the development of apoplexy.

Nevertheless, Wepfer and Morgagni having declared themselves partisans and even defenders of the opinion which I combat, I think it my duty to cite against them their own authority.

FIFTH OBSERVATION.

Valsalva reports the history of an old man, paralytic, who had a sanguineous coagulum lodged in the left ven-

* Messrs. Drs. Montaigu and Caillard, physicians of the Hôtel Dieu, were present.

† Memoir of Dr. Merat, Med. Society of Emulation, 7 vol. page 161.

tricle, and who sunk without having presented any of the symptoms proper to apoplexies.*

If Valsalva appeared to write wonderful things on this subject,† said Morgagni, you will find still more marvellous the case of Wepfer, related in the Sepulchretum.‡

SIXTH OBSERVATION.

A noble Polonese died without manifesting any apoplectic symptom. On opening the body a very considerable sanguineous effusion was found, not only between the cranium and the meninges but also even on the surface of the brain.§

Allow me to dwell one instant upon what we have already seen. If the effused liquids between the cranium and the brain, or in the interior of the ventricles, could by their presence determine the development of apoplexy, would not this disease have been manifested in the little daughter of Vesalius?

Must not the old man of whom Wepfer has related the history—he whose case has been written by Dr. Merat, have died of apoplexy? In fine, would the paralytic of Valsalva, and the Polish nobleman whose history excited as has been seen the wonder of Morgagni—could they possibly have escaped the apoplectic symptoms? Can a cause exist without an effect? Let us pursue the matter, and suppose for an instant that these fluids effectually produce apoplexies. Would it be possible to explain their history upon this supposition? How could we render an account of the apoplectic paroxysm? All the observers, if properly understood, have verified the fact that apoplexies are subjected in their progress to paroxysmal revolutions.

* De Sed. et Caus. Morb. Epist. V.

† Why wonderful? Because the celebrated Morgagni was impressed with an opinion, that there could not exist extravasated fluids in the brain without occasioning apoplexy.

‡ Sepulchr. Bonetus, schol. ad obs. vi. in addit. ad sect. ii.

§ Sepulchr. loc. cit.

Wepfer,* Drelincourt, Nyman, Werloff, Torti,† the celebrated Morgagni‡ and professor Portal,§ have, as regards the comatose complications of malignant intermittents, put this truth beyond doubt. Casimir Medicus, still further, has seen periodical apoplexies present the intermittent type.||

Now how shall we explain this fact on the supposition of compression? Shall we admit that in the interval between one paroxysm and the other, there may take place an absorption of the effused fluids, and a new exhalation for the paroxysm which follows? It has not been so stated: but still this would be the sole manner in which the phenomenon could be explained.

Apoplexies with effusion are susceptible of cure. This is a fact, which had become notorious in France some years ago, and Dr. Spurzheim¶ has recently again pointed it out. But Wepfer and Morgagni did not believe in the possibility of such a cure, because it was in direct opposition to their hypothesis,** which is in effect overturned by this sole consideration, that when the symptoms which occur in apoplexies with effusion are removed, what becomes of the fluid? Is it absorbed, and does it re-enter into the circulation in proportion as the cure advances? Or rather does it subsist after the cure? We have never examined this question, because it has always been thought, and is still thought, that apoplexy is a necessary effect of effusions: now here is what I have observed.

SEVENTH OBSERVATION.

An apoplectic person three weeks cured, contracted a violent pleurisy, and died the seventeenth day of this new disease. On opening of the cranium, there was coagulated blood of the size of a small egg lodged in the cortical substance of the great left lobe.

* Hist. Apoplect. p. 3, 18.

† See the works of these authors.

‡ Epist. III. IV. V.

§ See his work.

|| Periodical disorders, Apoplexies.

¶ See his work, art. Apoplexies.

** De Sed. et. Caus. Epist. II. No. 16.

EIGHTH OBSERVATION.

A hemiplegic patient was removed from the Hôtel Dieu to la Pitié on the 19th of April, 1816—he sunk into an adynamic state the eighth day after his entry into my department. The apoplectic attack which produced the hemiplegia had existed six weeks before his death. Since that time no symptom of apoplexy manifested itself. I opened the cranium, and found a sanguineous effusion lodged in the middle part of the great left lobe—the clotted blood was not so hard as in the preceding patients—its colour was of a grayish red.

NINTH OBSERVATION.

A woman, aged forty-one years, was brought from the Hôtel Dieu to the amphitheatre of la Pitié, and died of a puerperal peritonitis. A hard coagulum was lodged in the cortical substance of the right lobe. The inquiries set on foot discovered that this woman had had a "*coup de sang*" two years before.

TENTH OBSERVATION.

I opened the brain of an hemiplegic on the right side: after having met with an excavation of a small surface in the central part of the left hemisphere or lobe, I was surprised to find a hard coagulum of blood, of the size of a large nut, on the right lobe. The wife of the patient informed me that he had, three years before, an apoplectic attack, with feebleness (hemiplegia) of the limbs of the left side. I met with three artificial cavities or excavations in this man's brain—the most external was the effect of an attack of apoplexy, which occurred about ten years before. The symptoms of this disease did not appear until the second attack. Taught by these facts and a multitude of others perfectly analogous, that the effusions continued after the cessation of the symptoms of apoplexy, I particularly fixed my attention upon the new patients who presented themselves to my observation.

ELEVENTH OBSERVATION

Louis Gaudois, aged fifty-three years, was cured five days ago of a cerebral apoplexy, with a sanguineous cavity in the right hemisphere. I observed the course of cure with all the attention of which I am susceptible. An indigestion from fresh pork, with which unknown to me he glutted himself, carried with it the most serious consequences, and death on the twelfth day after the cessation of the apoplectic symptoms. On the opening of the cranium we found a bloody cavity lodged in the corpus striatum, of the size of a very small egg—the blood below was half liquid, nearly in the same state as that represented in the twelfth observation.

TWELFTH OBSERVATION.

On the 10th of February, 1817, I received at the hall St. Thérèse a lighterman (debardeur) of the Port St. Nicolas, who in going out of a tavern had a cerebral apoplexy, with a cavity in the left lobe. Scarcely was he cured when he procured some brandy, with which he intoxicated himself—he relapsed into another attack, with hemiplegia on the sound side, and died the fourth day of the relapse. We met with two sanguineous excavations in the brain, one in each lobe—the first, in the left lobe, occupying the middle of the centrum ovale, was an inch and a half in length by eight lines broad, and contained a coagulum of the same dimensions—the second, much more extensive, had destroyed the body of the corpus striatum of the right side, and filled with blood the ventricle of the same side.

I chose these out of twenty-two analogous cases, which I myself collected in my department at the Hospital of la Pitié, in all of which, the apoplectic symptoms being removed, the opening of the body shewed the clotted blood lodged in different parts of the encephalon. I join hereto that of a woman, sixty-one years of age, who survived an effusion thirty-seven days—who during this time preserved the exercise of her intellectual faculties to the last

moment, and in whom I found a considerable effusion, which had destroyed the corpus striatum and filled the corresponding ventricle.

Thus, though the apoplectic symptoms be removed, the effusions continue to exist. The effect of these fluids is, therefore, with man the same as that which we have proved as to animals. Neither the duration nor the severity of apoplexies, and still less their origin, is to be attributed to them: for how can it be supposed, that in sudden attacks there is caused instantly, without any previous signs, a sudden arterial rupture, or a copious serous discharge, if I may use the expression?

Last objection. If compression by fluids be the cause of apoplexy, a direct consequence is, that—*no apoplexy can exist without effusion.* Now, I am only embarrassed in the choice of authorities and facts, to prove the contrary. Varolius wrote in the commencement of the hypothesis, "*I have not found in the brain of apoplectics more serosity (excrementorum) than in that of persons who had died of other ailments.*"* The Doctors of Breslau were astonished at the demonstration of this fact†—and Quarin was so struck with it, that he exclaimed—"It seems that the cause of apoplexies removes itself with death."‡ Medicus, opening the brain of two apoplectics,§ and not finding any effusion, sought for the cause in the intestines. Tissot,‖ wishing to explain the same fact, which he met with several times, had recourse to a convulsive affection. Rahn,¶ Rega,** Scroerer, Kock,†† and the authors

* Morgagni, Epist. IV. p. 46.

† Admiratione tamen dignum est nulla evidens apoplexiæ, causa in defunctorum corpore post mortem deprehendatur. (Hist. Morb. Vrat. Apopl.)

‡ Velut si cum anima causa quoque mortis fugisset. (The work cited.)

§ Malad. Periodiq. Loc. cit.

‖ Epist. Med. varii, p. 80, 89, 90. Vid. Burserius, vol. iii. p. 86. Boucher, collection of Observations on Medicine, p. 77. Vid. Burserius, p. 87.

¶ Loc. cit. ** De Sympathia Cerebri et Abdominus, etc.

†† See his dissertation upon Bilious Apoplexy, in the collection of Scrverer, Veikart, and Lecat. (Kortum, Delect. Opusc. Medica de Frank,

on nervous apoplexies, entrenched themselves behind the sympathies of the brain with the different organs.

Let us deduce the fact from the midst of the explanations. There exist apoplexies without effusion. I have observed a great number of them, of which some will be reported hereafter.—1st. In the meningeal apoplexies without effusion.—2d. In the cerebral apoplexies, which terminate by inflammation or induration of the brain.

Thus apoplexies exist without effusions, and effusions without apoplexies. The sudden invasion of this disease, its periodicity often observed, its cure no longer left doubtful, the existence of coagula in the brain, without apoplexy, have been the results: in fine, the experiments upon living animals—all lead to a belief that the effusions are the effect, and not the cause of apoplexies—a very important conclusion in regard to the pathology of the brain, and to which I call the attention of observers.*

(*To be continued.*)

vol. vi.) Thierry has seen many apoplexies without effusions. (See Gai, on Apoplexy.)

* I think it useful to observe how untenable is the opinion of Wepfer and Morgagni on this fundamental point of apoplexies. After having admitted the idea of compression, and combated the facts which bore against it, Wepfer, convinced by his own observation, expresses himself thus:

"Quin etiam fateor, sanguinem extravasa copiosus intra cranium effusum, potius cerebrum ac cerebellum necessariis spiritibus vitalibus privandæ quam universam molem premendo ac coaretando." (Hist. Apop. p. 215.)

Thus the author rejects the cerebral compression as insufficient, in order to substitute his hypothesis as to the animal spirits. This hypothesis is now judged: therefore, of this passage there remains only the fact, that the compression by the effusions does not produce apoplexies. He has even gone further, in the following passage:

"Cætera vero quæ cerebrum premunt, sive intus prognata, ut tumores, calculi ossicula, telorum fragmenta et sililia sola ad apoplexiam producendam non sufficere facile credo." p. 230.

I have already said* how Morgagni, after distorting the facts, where the serosity was insufficient to apply them to the theory of compression, had had recourse to irritating acrimony. (Vid. Epist. Anat. Med. IV. p. 46. No. 1 idem, No 5. p. 49. idem, No. 2.)

* Page 252, orig.

ART. III. ***Cursory Remarks on Small-Pox, as it occurs subsequent to Vaccination.*** By GEORGE GREGORY, M. D. Physician to the Hospital for Small-Pox and Vaccination, at St. Pancras. Read Jan. 7, 1823.

THE acknowledged frequency of cases of small-pox subsequent to vaccination, in all parts of the country, is such as to have excited in no inconsiderable degree the fears of many and the anxieties of all. No one can look back upon the history of the last few years without feeling sensible that these unpleasant occurrences are on the increase, and it becomes therefore a matter not of curiosity merely, but of real necessity, to attempt some regular investigation of the subject. The difficulty cannot be met by mere reference to the fact, that small-pox once gone through, does not always secure to the subject immunity from a second attack. Cases of small-pox after vaccination are, beyond all comparison, more frequent than cases of *secondary* small-pox. The latter were at all times objects of curiosity even to the older members of the profession. There are few who have not seen repeated instances of the former. Within the walls of the Small-pox Hospital the latter were rarely seen. The former constitute at the present time a considerable proportion of the admissions into that institution.

With the view of illustrating this point, and also of showing how far the prevalence of small-pox after vaccination is on the increase, I have, in Table No. 1, given the total number of admissions into the Small-pox Hospital in ten different years, distinguishing such as occurred after real or *presumed* vaccination.* From this table it appears, that in the year 1810 the proportion of cases of small-pox succeeding vaccination, to the whole number of admissions,

* All cases here entered as having undergone vaccination, where the cicatrices were apparent, or failing that criterion, where the patient had a *distinct* recollection of the arm having risen and of the general progress of the disease. Such cases as were *known* to have failed are *excluded*.

was as 1 in 30—in 1815 as 1 in 17—in 1819 as 1 in 6—in 1821 as 1 in 4—and during the year 1822 as 1 in $3\frac{1}{2}$.

Above one hundred cases have occurred at the Small-pox Hospital during the last three years, the greater number of which fell under my own observation. Fifty-seven were admitted in 1822. The opportunities which have thus been afforded to me of observing small-pox, as it occurs subsequent to vaccination, have been tolerably extensive, and sanction in some degree the liberty which I presume to take of laying the results of my observation before the notice of my professional brethren.

The occurrence of small-pox after vaccination is a subject of very considerable pathological interest. Independent of its importance, as a question affecting the world at large, it affords curious matter of enquiry to the investigator of disease. It forms a link in that chain of facts which bear upon the general influence of the variolous poison upon the animal economy, and the several modifications of which it is susceptible—and to be thoroughly understood, it must be viewed in conjunction with them. The subject is one of no ordinary difficulty—but it is not my intention on the present occasion to enter upon it in that detail which I believe it to deserve. My remarks will be very cursory, and my principal design to put upon the records of the Society some evidence, that no fact which has reference to the great question of vaccine protection is overlooked or concealed.

I am thoroughly sensible of the extreme delicacy of this enquiry. The mere agitation of the question in a Society like this may be deprecated by some as altogether unwarranted and uncalled for—and from the distrust of vaccination which it seems to imply, calculated to occasion much serious evil. Were I not satisfied that this view of the subject is overstrained, I would not proceed. It is however clear to me that vaccination is now so well established, that no real danger can arise from examining, even in the strictest manner, every phenomenon connected with it. A large proportion of mothers, in the present day, were themselves vaccinated, and therefore the popular prejudi-

ces may now be considered as in favour of vaccination rather than against it. So far from anticipating evil, I look forward to the public good being benefited by the free discussion of the subject. Many persons have been brooding in secret over the failures of vaccination, and appear to have a fear of expressing their sentiments concerning it or of meeting the question in any way openly. To them the avowed investigation of the subject will I am persuaded prove satisfactory. But besides this, it is only by candid discussion that we shall ever be able to determine that highly important point, how far the failures of vaccination are owing to causes under our control—and how far therefore there exists a reasonable probability of obviating them, either wholly or partially, so as to increase the security of the vaccinated.

I shall first enquire in what manner and to what extent, the effects of the variolous poison upon the animal economy are modified by the influence, previously exerted, of the vaccine virus. I shall then offer a few reflections on the causes of the occurrence of small-pox after vaccination, and on the sources of difference in the degree of modifying influence which vaccination exerts.

It is almost unnecessary to remark in the first place, that in a very large proportion of cases, the same immunity is afforded by vaccination as by once undergoing the genuine variolous disease. What the *exact* proportion is we are unable to ascertain. It does not even appear that any approximation to the truth, which can be much relied on, has yet been made. We may even go further, and be warranted in saying, that no calculations tending to establish this point, which are made in the present day, can reasonably be expected to hold good in future—and the reason is obvious. The failures of vaccination are now far more numerous than they were ten years ago, and no certainty exists that they have yet reached their maximum.

In cases where the vaccine virus fails to impart a *perfect* security from the future influence of the variolous poison, it serves at least to modify *certain* of its effects. These it is important to investigate.

1. Vaccination does not appear to lessen the violence or shorten the duration of the first or eruptive stage of fever, which is generally as severe and even sometimes severer and longer in its duration than that of the casual confluent small-pox.

2. It does not appear in like manner to influence the *quantity* of eruption upon the skin, so much at least as has been generally imagined. It is true that in many cases of small pox subsequent to vaccination, the eruption has been very scanty—but in a large number also I have seen it very copious, more particularly about the face, breast and upper extremities, and occasionally fully equal in point of *quantity* to what is seen in the worst kinds of confluent or coherent natural small-pox.

3. The great power of vaccination unquestionably consists in modifying the *progress of inflammation* in the variolous eruption—and here it cannot fail to attract observation, how strikingly opposed to each other in this respect are the influences of inoculation and of vaccination. Inoculation lessens the *quantity* of eruption, but does not alter in the slightest degree the progress of inflammation in that which *is* brought out. Vaccination, on the other hand, while it does not sensibly affect the *quantity* of eruption, always influences more or less the progress of inflammation, however copious the eruption may be. The same desirable result, the diminution of mortality, is obtained in either way. By checking the quantity of eruption or the degree to which inflammation in it extends, the disease is prevented from bringing on those impediments to the functions of respiration and perspiration, which occasion secondary fever and endanger life.

In all or nearly all cases of natural and inoculated small-pox, the eruption proceeds to ulceration more or less superficial according to the violence of the disease—and the ulcers heal by the common process of scabbing and cicatrization. In cases of small-pox however, subsequent to vaccination, the cutaneous inflammation is checked at so early a period, that the fluid in the vesicles seldom reaches

the state of pus, the cutis vera is never ulcerated, and consequently the healing process takes place by the conversion of the vesicles into tubercles and their subsequent *desquamation.* This constitutes a very well marked and important character of the vaccine or modified small-pox. A similar modification of the variolous inflammation of the fauces and trachea undoubtedly takes place—but the exact nature of the difference it is in this case more difficult to define.

4. Though vaccination modifies in a large proportion of cases the progress of inflammation in the skin and throat, it is curious to observe that it does not always affect the course of the disease when the variolous poison fixes itself on other parts, more particularly on the brain.* It is in this manner that small-pox after vaccination occasionally proves fatal.

The following cases will illustrate the position which is here advanced.

CASE I.

Variola succeeding vaccination, modified in as far as regards the eruption, but proving fatal by affection of the brain.

William Timms, æt. 30, labourer in lead works, in the habit of frequent intoxication, was admitted a patient into the Small-pox Hospital, Sept. 21, 1820. He had been vaccinated by Mr. Griffin, surgeon, of Deddington, Oxfordshire, eighteen years previously. Two vaccine cicatrices were very apparent on the left arm. The eruption proved to be distinct, and on the seventh day it was drying on the face and exhibiting the usual tuberculous character of modified small-pox. Throughout the whole course of the

* I may be permitted perhaps to remind those who have not been in the habit of seeing small-pox lately, that the eruption on the skin and throat is only one of the effects of the poison. Another at least equally important, both with reference to pathology and practice, is that which is exerted upon the brain and nervous system—the chief evidences of which are delirium, inflamed eyes, stupor or restlessness, and disposition to erysipelas and gangrene.

disease however there were some obscure marks of affection of the brain. On the subsidence of the cutaneous inflammation, comatose symptoms came on and he died on the following day.

The brain on dissection exhibited no appearances of recent disease.

CASE II.

Variola succeeding vaccination, modified in as far as regards the eruption, but proving fatal by an obscure affection of the brain.

Ruth Beddoes, æt. 19, of gross and plethoric habit of body, was admitted into the Small-pox Hospital, September 28, 1822, with a distinct and mild eruption, attended however with considerable conjunctival inflammation. She had been vaccinated five years previously at Bishop's Castle in Shropshire. The arm *inflamed severely*, and for a long time afterwards was kept in a sling. A very small vaccine cicatrix was observable in her left arm.

The cutaneous eruption passed through its stages rapidly—but the appetite never returned. On the eighth day ophthalmia came on with great violence, yielding however to the free use of the lancet. Five days afterwards the arm in which she had been bled became affected with severe erysipelas, which subsequently attacked the leg of the opposite side. Four days after this, violent delirium supervened and she gradually sunk into a state of low typhus, in which she died. On opening the head no morbid appearances of any kind could be detected, nor were there any traces of disease in the thorax or abdomen.

Having thus described the manner in which the effects of the variolous poison are modified by previous vaccination, I proceed to notice the *degree* to which such modification takes place. This varies very greatly—to an extent indeed hardly conceivable by those who have not paid minute attention to the subject. Sometimes the disease after vaccination, is so *highly* modified that physicians can

scarcely think themselves warranted in calling the complaint small-pox. It has all the characters of *varicella*, and has even been confounded with those slight papular eruptions which are met with in particular habits and in irritable skins, more especially in warm weather.

On the other hand, the modification produced by previous vaccination is in some few cases so *trifling* as hardly to be perceptible. Between these extremes every possible gradation has been noticed—as the practice of the Small-pox Hospital during the last year has abundantly testified. It is highly satisfactory to know that the instances of *complete failure* from well ascertained vaccination, are very few in number, and will hardly bear a comparison with those numerous instances in which the disease was so modified by it as to preclude *all* anxiety for the patient's safety. Of the fifty-seven cases of small-pox after vaccination, admitted into the Small-pox Hospital in 1822, forty-four were discharged in perfect health *within* fourteen days from the period of their admission.* There were five fatal cases. The history of one of these (Ruth Beddoes) has been already detailed—and it is evident from the reported state of the arm, that the vaccination in this instance was not trustworthy. It is moreover fair to presume, from the *high* degree of inflammation which attended the process of vaccination, that there was a strong *predisposition* in this constitution to suffer from the variolous poison, and the result would perhaps have been equally fatal had the patient taken small-pox by inoculation. The same degree of doubt attaches to the remaining four. In two of them the cicatrices were very large and irregular. In one, *no* cicatrix was discernible—and in one only could it be said that the scars were tolerably regular.

In the present state of the country, it appears highly desirable that some investigation should take place into the

* Table No. 4 exhibits an abstract of the periods of their duration in hospital.

real causes of the occurrence of small-pox after vaccination, in order to determine if possible how far there is any probability of our being able to obviate them in future.—The following observations are thrown out with the view of assisting in the determination of this question:

1. Small-pox after vaccination unquestionably prevails in particular families—showing that in them there exists some peculiar susceptibility of the variolous poison. Various nstances of the kind have fallen under my own immediate observation, the most striking of which is the following:

George Ferriman, æt. 30, was admitted into the Small-pox Hospital, Oct. 28, 1822, with *pretty severe* modified small-pox—and with him were also admitted his two children, Thomas and Harriet, both labouring under a *very slight* form of the same disease. The father had been *inoculated for small-pox* when a child, and was always considered to have passed through the disease in a regular way. His children had been *vaccinated*, and their arms exhibited very perfect cicatrices.

I have seen small-pox attack three individuals of the same family, who had been vaccinated at different ages, in different places, and by different persons. I witnessed this during the last summer in the family of a medical practitioner, in the neighborhood of Red Lion-Square.

2. It is certainly worthy of observation, that the great majority of cases of small-pox subsequent to vaccination which have occurred at the Small-pox Hospital, have been persons between the ages of 15 and 21. Nineteen is the average age of the whole, as the table No. 2 will show.—How far this may depend upon accidental causes or upon the length of time which has elapsed since the general diffusion of vaccination, I am unable to speak decisively. From the fact however that many of these persons had been frequently and thoroughly exposed to the contagion at former periods of their lives, I am induced to entertain the notion that there is something in the habit of body peculiar to that age, which renders the system more than usually disposed to suffer from the influence of the variolous poison.

The circumstance now adverted to has lately become obvious to the world, and it has revived an opinion entertained in the earlier days of vaccination, that its influence on the system wears out in the progress of life and requires periodical renewals. The notion has latterly been acted upon to a great extent—but I have not been able to ascertain that the results of *revaccination* correspond with the theory which leads to it.

3. In any investigation of the causes of small-pox subsequent to vaccination, it would be improper to overlook the remarkable connexion that subsists between the degree of perfection in the vaccine cicatrix and the violence of the secondary disease. This important fact was forced upon my attention by the results of the last year's experience at the Small-pox Hospital. It is indeed in opposition to the opinion entertained by several authors of acknowledged reputation—but the extent of my opportunities enables me to speak with much confidence on this point. When the scar on the arm is perfect—that is, distinct, circular, radiated, and cellulated—but above all, when it is small, so that it may be covered by a pea—the secondary affection (if from peculiarity of habit or any other less ascertained cause it does occur,) will be slight, and hardly deserve the name of a *disease*.

On the other hand, whenever the scar is large and bears the marks of having been formed by high local inflammation, and wants the other distinctive characters just enumerated, the chance of small-pox occurring in after-life will be greater, and, *cæteris paribus*, there will be a stronger likelihood of its proving severe.

This principle receives a striking confirmation from what takes place in *revaccination*. Where the cicatrix is perfect, it is impossible or nearly so to reproduce the vaccine disease in any thing like its genuine form. In proportion to the imperfection of the cicatrix will be the degree of approximation of the *second* to the *primary* vaccination.

These considerations tend to establish, *as a pathological*

principle, that the occurrence of small-pox subsequent to vaccination is dependent upon the *intensity of the vaccine influence, as primarily exerted*—and they lead to the belief that the appearance of the cicatrix may be taken as a *measure of that intensity.*

4. The last point to which I am desirous of directing the attention of the Society, is the fact that a very large proportion of those persons who have been admitted during the last three years into the Small-pox Hospital, having the disease subsequent to vaccination, had been vaccinated in the country. This may be considered perhaps as an *accidental* circumstance. It may be argued that the domestic servants in London, who of all persons may most naturally be expected to resort to an hospital in the time of sickness, are in a great measure supplied from the country. It may possibly be owing in part to the susceptibility of the disease being increased (here, as in other instances) by change of air—but making all necessary allowances, the disproportion between those who take small-pox after vaccination in the country, and after vaccination in London or some other large town, still appears to me so great that we must seek for an explanation which may have a wider range of influence. I have long been impressed with the notion that practitioners in the country have frequently vaccinated with lymph which was *not perfect* in its qualities. I am inclined to entertain this opinion, first, from having been able to trace several cases of small-pox after vaccination to *particular* villages in counties bordering on the metropolis—and secondly, from having observed that a great proportion of those admitted into the Small-pox Hospital after country vaccination, had *large*, *irregular*, and therefore *imperfect* cicatrices.

If the notions which I entertain of the influence of vaccination on the animal economy and on the causes of its occasional failure are correct, their general diffusion might assist, not only in upholding vaccination as an object of great national and individual importance, but in checking

those unpleasant occurrences which are now making such alarming inroads on the confidence of the public in the vaccine protection. It would induce practitioners to revaccinate with fresh and genuine lymph, those whose arms do not exhibit the perfect cicatrix—it would lead those in the country to apply more frequently to large towns for a supply of recent lymph—it would point out the propriety of their putting less confidence than heretofore in points and preserved lymph—and it might impress upon all the *indispensable* necessity of a close attention to every part of that process which, though of trifling importance at the moment, is yet of incalculable consequence to individuals in every future period of their lives.

APPENDIX.

NO. I.

List of patients having small-pox after vaccination, admitted into the Small-pox Hospital, in two different periods, of five years each—showing the proportion such cases bear to the whole number of admissions.

Year.	Total admissions.	Total of Small-pox after vaccination.	Proportion of cases of Small pox after vaccination, to the whole number of admissions.
1809	146	4	One in 36
1810	149	5	30
1811	94	6	15
1814	79	4	20
1815	101	6	17
Total in five years.	569	25	22
1818	58	9	6
1819	97	17	6
To Sept. 7. 1820	142	25	6
1821	117	28	4
1822	194	57	3½
Total in five years.	608	136	4½

NO. II.

Table of the ages of the different patients affected with small-pox after Vaccination, who have been admitted into the Small-pox Hospital during the last five years.

Under 10 years of age,	5 persons.
At 11 - -	2
12 - -	1
13 - -	2
14 - -	5
15 - -	3
16 - -	7
17 - -	14
18 - -	13
19 - -	11
20 - -	18
21 - -	13
22 - -	9
23 - -	10
24 - -	9
25 - -	4
26 - -	3
27 and upwards,	7
	136

NO. III.

List of patients having Small-pox after Vaccination; distinguishing those vaccinated in the country from those vaccinated in town, and those not ascertained.

Year.	Vaccinated. In the country.	In London.	Not ascertained where	Total Vaccinated.
1809	3	1	0	4
1810	4	0	1	5
1811	5	0	1	6
1814	4	0	0	4
1815	5	1	0	6
1818	7	0	2	9
1819	11	1	5	17
To Sept. 7. 1820	6	1	18	25
1821	19	2	7	28
1822	35	6	16	57
Total.	99	12	50*	161

NO. IV.

Table, exhibiting the duration in hospital of the cases of small-pox after vaccination, admitted into the Small-pox Hospital during the year 1822.

Less than 7 days,	12 persons.
——— 14	32
——— 21	6
——— 6 weeks	2
Fatal Cases	5
Total	57

* There is every probability that the fifty persons set down in this column were vaccinated in the country—as *residents* in town would hardly have expressed any doubts on the subject.

Art. IV. *State of Medicine in Spain after the expulsion of the Arabs.* By Don Garcia Suelto, M. D. of the Faculty of Madrid, late Member of the Supreme Council of Health of Spain, and Member of several Academies. Translated for this Journal by R. La Roche, M. D.

In historical researches into Spanish Medicine, the first object to the examination of which we should direct our attention, is the Medicine of the Arabs. But in order to arrive sooner at the subject of the actual state of this science in Spain, I have thought it advisable in the sketch I am about presenting, to suppress this part, which might perhaps be regarded as foreign to it, and to confine myself to what belongs exclusively to my nation.

The war carried on in Spain in order to throw off the yoke of the Arabs, and the subsequent expulsion of all the Moorish families settled in that country, seemed from all appearances well calculated to put an immediate stop to the progress of the sciences, and to occasion some impediments in the study of medicine. Nevertheless when we trace the literary history of the centuries subsequent to this period, we easily discover that our science continued to be cultivated with success—that the zeal of physicians did not diminish, and that genius and the love of the science served as substitutes for the absence of the ancient professors.

The bibliographical notice which I here offer, is a simple extract from the long list of Spaniards who have written on medicine. No branch of the healing art—none of the subordinate studies, escaped their attention—and in presenting some of the most important discoveries which we derive from them, and which others have appropriated to themselves, it will be proved that so far from neglecting the reading of their works, we may collect from them numerous and interesting facts. This perusal will render us more just—it will serve to remove prejudices injurious both to the science and to those who are the objects of them—and which originating as they do from our ignorance, are disgraceful to ourselves. Physicians are already acquainted with some of the Spanish writers, but they have never taken the trouble to enter into a critical examination of them—and have contented themselves in their investigation with the information derived from a common catalogue. Hence bibliographers have not shewn sufficient eagerness to procure printed copies of their works or to examine their manuscripts, which up to this period constitute not the least interesting part of several libraries, and more especially of the Royal library of Madrid.

It is a fact placed beyond the possibility of doubt, that soon after the expulsion of the Moors, the schools were re-established—and that they even existed before that period, and independently of any assistance from these latter. This may be learned from the expressions of the learned critic,

Peter Chacon, in his *History of the University of Salamanca.* "The physicians," says he, "who taught in it, had endeavoured to re-establish the art of medicine, which at that period was lost in all Europe, with the exception of that part of Spain occupied by the Arabs. Those physicians who understood the Arabic language, from a frequent intercourse with their neighbours the Moors, had derived from them a part of their knowledge in the science—they began to teach and practice Methodical Medicine, by establishing it on philosophical principles."

The greater number of Spanish physicians have written in Làtin, and are distinguished by the purity and elegance of their style, as well as by an intimate acquaintance with the Greek language, and a knowledge of fine literature. These acquirements are sufficient to prove that their literary education had not been neglected. We have in consequence, the translation of *Amphitrio*, and a poem on Medicine by Doctor Villalobos—the translation of the entire works of Seneca by Pinciano—of Sallust by Laguna—the interpretation of the word *acia* of Celsus, by Alphonso Nugnez—the translation of Dioscorides printed in Salamanca in 1566, &c. &c. We must in addition to these cite as specimens of good latinity, Gutierres of Toledo, whose work entitled *De regimine potus in lapidis præservatione*, appeared in 1494, and was afterwards published by himself in the Spanish language. The *Elaborationes Anatomicæ* of Nugnez de la Gerba, professor of anatomy, a title which attests the establishment, at that period, of the chairs of anatomy—the works of Laguna, of whom we shall have occasion to speak, and who possessed in an eminent degree a knowledge of the Greek language—those of Pinciano under the title of *Observationes in loca obscura aut depravata Historiæ naturalis Caii Plinii, Cum retractationibus locorum quorumdam Geographicæ Pomponii Melæ*, published in 1544, (this work proves that the study of natural history was not then unknown to our physicians,) and his translation of Thucydides on the plague of Athens, the precious manuscripts of which remain in the possession of my wor-

thy friend Doctor Luzuriaga. Finally we must cite also the writings of Huerta Villalobos, Estive, Sepulveda, and Gaspard Caldera de Heredia, without including numerous commentators of Hippocrates, Galen, Paulus Eginetta, Diascorides and the Arabs.

Orozco excelled in a knowledge of the Greek. At the early age of twenty-one he pointed out the errors committed by the translators and interpreters of Paulus Eginetta, and afterwards of Ætius. His *Annotationes in interpretes Aetii* were printed in 1538, and it is only to the unfortunate abandonment in our days of this learned idiom that we must attribute the want of attention paid to the work of Orozco. It is not my intention by these remarks to exaggerate the necessity of the study of the Greek language—but we must readily perceive that the most trifling error in the translation occasions very serious consequence, and disfigures the doctrine of an author. Foës, Chartier and Vanderlinden, in confounding the Greek word *fevret* with *olet*, have afforded us an example of the danger of these errors.

When we direct our attention to the medical part of Spanish literature, we readily discover that the science is under obligations to the physicians of our country for the transmission of the Hippocratic doctrine, and in general of that of all the medical patriarchs. The most remarkable among the translators and commentators in medicine are, Lopez Pinciano, the famous Valles (whose work underwent two editions in France, one in Orleans and the other in Paris in 1663,) Christopher de Vega, Rodrigo, Fonseca, Bustamente de la Paz, Vega, Juan Bravo, Alphonzo Lopez, Michel de Heredia, Antonio Zamora, Lazare de Soto, Ponce de Santa-Cruz, Jeromio Ximénez (whose writings have merited the esteem of the celebrated Barthez), and more than fifty others already too well known to necessitate here a detailed enumeration.

More than sixty others who have really illustrated the art by their writings, occupied themselves at the same time with preserving and commentating on the doctrine of the

Arabs, and particularly on that of Ebu-Sina, or Avicenna. The mos deserving of attention among them are, Diégo Lopez, Gabriel de Taraga, Jacques Lopez, Ledesma, Garcia Carrero, Perez de Herrera, and Antonio Blessa.

The catalogue of Don Nicholas Antonio alone contains more than three hundred authors who have written on the various parts of the healing art—and whose works and names have passed to posterity—namely, more than one hundred treatises on natural philosophy, mathematics, natural history and chemistry—without comprehending our numerous commentators on Aristotle—six classical works on veterinary medicine—sixteen on hygieia—twenty treating expressedly of drinks and baths—more than twenty more on the elementary institutes of medicine—eight on midwifery and the diseases of women—six on prognostics and symptoms—more than thirty on fevers, the pulse, and the urine—upwards of forty on anatomy—four on the physical education and diseases of children—upwards of sixty on diseases in general and in particular—sixteen on bleeding and purgatives—thirty on surgery—forty on the materia medica and pharmacology.

I do not include in this enumeration a considerable number of very curious monographs and miscellanies, on different points of the art. Nor shall I here present the names of all those writers, since they may be readily found in the work of Antonio, and would still form a very incomplete list. It would moreover prove to be a labour as useless, as a complete analysis of these writings would be long, and out of place in so general a sketch as the present. It will suffice to offer here a very cursory examination of a small number of the above enumerated books.

The establishment of a supreme and independent tribunal for directing the teaching, administration and exercise of medicine, dates from the happy reign of Elizabeth the catholic, from the year 1474 to 1504. This same sovereign caused the formation of the legal code of the faculty, which has subsisted even down to our days, and of which we shall speak in another place. Among the writings of Mercado,

published in the sixteenth century, we find a work which proves the progressive improvements this noble institution underwent—it is entitled *Institutiones jussu regis factœ pro Medicis in praxi examinandis ad faciendam Medicinam.* In it we find the regulations enforced in the graduation of physicians, the means resorted to in order to obviate the facility of procuring assistance from others during examinations, and to prevent the despotism or partialities of the judges. Nor does it appear less evident that the same regulations were enforced in regard to the practice of medicine, since there still remains a work, by Rodriguez de Fonseca, treating expressedly of consultations, and entitled *De consultandi ratione.*

The same Mercado who published two treatises, 1st. *De febrium essentiâ, causis, differentiis et dignotione*—2d. *De pulsûs arte et harmonia*, is undoubtedly the first who occupied himself with the description and treatment of malignant fevers. On this subject, I must direct the attention to the treatises of Christopher Diatristan de Acuna, Gaspard Bravo Ramirez, Ferdinand Cardoso, Francis Valles, Ferdinand Mena, Lazaro Gutierrez, Onofre Bruguer, Alphonzo Lopez de Corella, Bocangel, Longas, Carthagena, Caldera de Heredia, Antonio Castro, Sebastiano Soto, Rodriguez de Fonseca, and Abraham Nehemias, the greater number of which have been totally overlooked in the modern monographs of fevers. It is perfectly familiar to every one that Solano de Luques led the way to Nichell, Bordeu, Fouquet, Hunauld, &c. But one century before the publication of his *Lapis Lydius Appolineus*, there were already other treatises, the existence of which is unknown to the writers on the pulse—such are those of Antonio Ponce de Santa Cruz, of Christopher de Vega, and Alphonso Nugnez, *De pulsuum essentiâ, differentiis, cognitione, causis et prognostico.*

The appearance in Spain, in the beginning of the present century, of the disease under the name of yellow fever, elicited from the most distinguished men of the art very interesting researches. They were seen to travel in a great measure alone, in a path hitherto unknown

to them, but which had however already been clearly traced by the Spaniards. Had they been more familiar with the history of Spanish medicine, they would not have neglected to make use of the fine descriptions and important illustrations found in the writings of Antonio de Fonseca on the *plague*, on *contagious diseases*, and on the *epidemical fever* of 1621—in the treatises *De Peste*, by Laurent Brandaon and of Sebastiano Nuñez—in that of Paul Correa, entitled *De causis* et *curatione pestis*—and in the dissertation of Emmanuel de la Cerda, *contra pulverem venenosum*, printed in Milan in 1621.

The explanation of the frequent conversions of disease, appeared also to be the natural effect of the modern application of philosophy to medicine. Already however it was due to the genius of a Spaniard, Stephano Rodriguez de Castro, of whom we possess a manuscript entitled *De mutatione morborum in alios*. There exists from him a work on the same subject, the execution of which however is feeble.

It is sufficiently well known that Mead published his work on sacred medicine in the year 1749. The Spaniard Vincent Moles, however, had already published his *De morbis in sacris litteris* in 1642—and Valles, a treatise, *De iis quæ scripta sunt physice in sacris litteris, sine de sacrâ philosophiâ*, in 1595.

Anterior to those who are regarded as the first writers on the croup, and particularly before Dr. Home had published in Edinburgh, in 1765, his researches on the nature, causes and treatment of croup, or cynanche stridula, there had already appeared in Spain in 1611, a Latin treatise on the *Garrotillo* (name given in Spain to that distressing disease) by Alphonso Fontecha—and also one by Christopher Perez de Herrera, printed in Madrid in 1615, under the title of *De Morbo suffocante, vulgo Garrotillo.* Independently of these, several treatises on the same subject had appeared in the Spanish language, by P. Rotundis, Andreas Tamayo, Ferdinando Sola, Francisco Peres Cascales, Juan Soto, and Nicholas Gutierrez de Angulo.

Certain uncommon or imperfectly understood diseases had not escaped the attention of our Spanish physicians, and their talents had been exercised on several of the most curious and interesting points of philosophy. In order to be convinced of this truth, it is only necessary to examine attentively the following works: *Consultatio de Plica Polonicâ*, by Rodriguez de Fonseca, professor of the practice of medicine, printed at Frankfort in 1625, 1 vol. 12mo.—the books *De fascinatione*, by Antonio de Carthagena, and by Juan-Lazare Guttierrez—the treatises *De Mutatione Æris*, by Diego Palomino, and *De formalis præscribendi medicamenta*, by Francisco Sanchez—*Controversiæ medicæ et philosophicæ, by* Valles—*De viri et fæminæ comparandâ fecunditate*, by Gabriel de Tarraga—*De variâ rei medicæ lectione*, by Garcia Lopez—*Eliseus secundarum questionem campus*, de Gaspard de los Reyes, likewise very much esteemed for their purity of style—The *medical observations* by Valeriola—*De naturæ maliciâ ætatem superante* by Jeromio Pech—*De marsis et psyllis* by Juan Bravo—*De Signis Veneni Sumpti* by Balester—*Medical Questions* by Jeromio Ximenez—*Difficiles disputationes variæ*, and the *Consultations on complicated diseases* by Mercado—*Selectæ philosophiæ et medicinæ difficultates* by Rodriguez de Pedrosa —*Medicæ Disquisitiones* by Guttierrez Andrade—*Exercitationes de animalibus microcosmicis*, by Rodriguez de Castro, &c. &c.

Nor was the study of anatomy either neglected or limited, as many have maintained, to the doctrine of Galen alone. In support of my assertion, I shall cite among others the writings of Alphonso de Guevara, *De re Anatomicâ*—the anatomy of Francisco Salat—that of Laguna, published in Paris in 1535—of Juan Lobera d'Avila, and of Juan Alos—the *Summa Anatomica* of Juan Burgos, and P. Ximenez. It is necessary to observe, that this latter in his work printed in Valencia in 1559, styles himself the pupil of Vesalius, and dedicates to him the discovery of the third bone of the ear, which he was the first to make. He

expresses himself in the following terms: *dico Vesalio præceptori nostro—tertium illud ossiculum repertum est a me frequenter*, &c. &c.

Dr. Alderete, who in 1535 and 1536 was professor of medicine in Salamanca, must be regarded as the inventor of wax bougies for the examination and treatment of strictures of the urethra. Alderete taught this discovery to his pupil Amato Lusitano, who makes the same avowal in the fourth book, cure 19th. Amato wrote his books at Ancona in 1552 and 1553, and dedicated them to the grand commander of Portugal, don Alphonso Alencastro. He affirms, that by this means he had cured in Lisbon, an officer twenty-five years of age, and cites as his witnesses, Lewis Nunez Coimbre and George Henriques, both physicians of eminence in that city, and the astronomer Emmanuel Linda. He further affirms, that he had imparted his method to a more experienced than learned surgeon, named Philippe, the same who attended the officer, and who had requested him to undertake the treatment—that Philippe thus acquired the secret, and having gone to Rome, appropriated it to himself, by leading into error Dr. Laguna, who in his work entitled *Methodus cognoscendi extirpandique excrescentes in Vesicæ collo Carunculas*, eulogizes that surgeon as the true inventor of the bougies. It is therefore probable, that Ambrose Pare was likewise led into a mistake in attributing this invention to the surgeons of Montpellier. Dr. Christopher Vega had announced this same remedy in 1588, in his treatise *De Curatione Caruncularum*, and we are not a little surprised to see Andréas Aleazar, a physician in all respects very learned, already on the point of making the discovery, lose sight of it, as may be learned from the following expressions contained in his work on the *internal ulcers of the urethra, which impede the passage of the urine*, (5, 25, p. 209 and 210). *Oportet ergo tùm foramine ulceris Jam satis mundificato cavam plumbeam tentam tenuem per urinarium meatum subtiliter intromitti, et indè per aliquot dies urinam excerni, ne ulceris osculus in transitu ab eâ tan-*

gatur, videlicet enim virtute et potentiâ acrimonia aglutinationem prohibet.

But the same Alcazar is not less deserving of the esteem of posterity by his other labours. In the year 1525 he had invented for the difficult operation of the trephine, several instruments more convenient and advantageous than any of those known before. The figure and plate of these may be found in his treatise *De Vulneribus Capitis*, *lib.* 1, *cap.* 16, *p.* 60, *et seq.* In this work he relates that being at Guadalaxara, the place of his nativity, the king of France, Francis I. passed through that city, accompanied by a surgeon highly distinguished and worthy the entire confidence of that monarch, and who was lodged in his house: this circumstance afforded him the opportunity of showing these instruments to this colleague, who complimented him very much, and promised, regarding them as he did, either as more perfect, or better than those made use of before, to introduce them into public notice in France on his return to that country. At the same time Louis de Lucena, natural philosopher and physician, who possessed much genius, and had aided the author in the invention of these instruments, went to Italy, where he suggested the idea of them. During the twenty years he remained in that country, having frequent intercourse with the most learned professors of surgery, (a science he was particularly fond of) he made them known. In this way their employment became almost universal. Notwithstanding which, however, Vidus Vidius claimed them thirty years after as his own invention, though without endeavouring to present them in an exact and faithful manner.

In perusing the various authors who have spoken of the venereal disease, such as Andreas de Leon, J. Almenar, Cyprien Maroxa, Lewis Ysla, Lewis Mercado, Michel Pascal and others, we are convinced that it is to the Spaniards the profession and humanity are indebted for the introduction of mercury in the treatment of that disease. This discovery has been falsely attributed to Van Helmont, and we are not a little astonished at the injustice or forgetful-

ness of those who have searched elsewhere for the historical proofs of the origin of this disease.

I shall close this bibliographical sketch with citing the writings of a Spanish lady, which undoubtedly prove that a taste for the physical sciences was pretty generally diffused throughout Spain, and that some honour was attached to their cultivation, since these studies formed also the finest ornament of the fair sex. Donna Oliva de Sabuco, born in Alcaraz, published in Madrid in the year 1588, 1st, *The New Philosophy of the Nature of Man*—2d, *Conversations on Self Knowledge, in which are introduced advices, in order that man may know his nature and the natural causes of life, and of premature and violent death, &c.*—3d, *Brief Treatise on the formation of the World*—4th, *Of those things which ameliorate this World and its different conditions*—5th, *True Medicine and true Philosophy*—6th, *Remedies of the true Medicine*—7th, *Dicta brevia circa naturam hominis et mundi antiquis occulta.* A third edition was printed at Prague in 1622.

Spanish medicine, at all the periods I have mentioned, has been particularly honoured and held in high estimation. The establishment of a special tribunal of the faculty on the same footing with the other privileged courts of justice—the formation of a penal code—the zeal displayed by the government for the promotion of the science—the great number of illustrious writers on medicine, who have always seen their labours honoured—are the most conclusive proofs of the protection and respect enjoyed by the profession. Of this, however, we may be further convinced, by reading a work by Marc Garcia, entitled "*Honour of Castilian Medicine and Surgery*:" and history offers us further proofs of this in the honours paid to Lewis Lobera d'Avila, physician to the emperor Charles the Fifth—to Lewis Mercado, chief physician of Philip the Second and Third—to Antonio de Carthagena, physician to the French princes sent to Spain as hostages for their father Francis the First—to Andree Laguna, who, descending from a distinguished family of Segovia, added to his nobility the titles of Roman Knight and Count Palatine, given him by Pope Julius the Third, who appointed

him his physician—to Antonio Ponce de Santa Cruz, president of the Medical Tribunal, who received from the king the donation of an abbey, similar to those given formerly to the princes of the blood*—to Gonzalvo de Toledo, physician to the Queen of France in 1508—to Francis Valles, &c. &c.†

As we have presented it, such is the brief sketch of the art of medicine, in Spain, in those ages succeeding the expulsion of the Arabs. I have cited but a small number of the authors who have written on that subject, besides those whose names are found in the catalogues and codes already mentioned. There are many others whose names have hitherto escaped the researches of historians. I myself had several, which however, together with all my library, I have lately lost. Dr. Franseri, a very distinguished physician, was likewise in possession of a great number. But the most scarce and complete collection is that of Dr. Luzuriaga. This physician, already very advantageously known in Europe, is at present occupied in the composition of a memoir, in which he will show that the discovery of the circulation of the blood, attributed to Harvey in 1628, belongs to the Spanish physicians who announced it in the years 1535, 1542 and 1549, and demonstrated it in the most satisfactory manner in 1551 and 1598. I shall take the earliest opportunity, as soon as this interesting work appears, of making it known. Dr. Luzuriaga had already inserted in the first volume of the transactions of the Royal Academy of Medicine of Madrid, another memoir, in which he proved that the invention of making sea water fresh, belongs exclusively to a Dr. Laguna.

In respect to the many treasures which America afforded to the medical sciences, they have not, as many have thought it to imagine, been overlooked. I shall however reserve for a future occasion this too extensive subject, and will here content myself with pointing out to the attention of

* Don Bermudo, cousin to king Alphonso the Sixth.

† See the *Bibliotheca Hispana*, by Nicholas Antonio, already quoted in the course of this article.

critics, the Natural History of the three kingdoms of nature in Mexico, by Hernandez. It is necessary to observe that this work is not noticed in the translations and commentaries of Clusius, Rech and Linceus, the last having been printed in Rome in 1651—consequently we should consult in preference the Madrid edition of 1790, printed on the original and under the direction of D. Gomez Ortega.

Art. V. *A Sketch of the History of Mineralogy.* By Isaac Lea. Read before the Academy of Natural Sciences of Philadelphia, November, 1823.

In the ensuing Sketch of the History of Mineralogy, I propose to give some account of the science from the earliest period to the present time, regarding more particularly the works of the older mineralogists.

The various books of the Bible are certainly the most ancient that have reached us—and chapter v. of Genesis informs us that *Tubal Cain*, the eighth lineal descendant of Adam, was "an instructor of every artificer in *brass* and *iron*." This book, however, speaks of the existence, not of the discovery of the metals. In Deuteronomy, chapter viii. *Moses* promises the Israelites "a land whose stones are *iron*, and out of whose hills you may dig *brass*." At a very early period, the precious stones were in great request. *Job*, chapter xxviii. speaking of wisdom, says "it cannot be gotten for *gold*, neither shall *silver* be weighed for the price thereof. It cannot be valued with the *gold of Ophir*, with the precious *onyx*, or the *sapphire*. The *gold* and the *crystal* cannot equal it—the exchange of it shall not be for *jewels* of fine *gold*—no mention shall be made of *coral* or of *pearls*, for the price of wisdom is above *rubies*. The *topaz* of Ethiopia shall not equal it, neither shall it be valued with *pure gold*." We find, by the chronological tables, that *Job* and *Moses* lived about 1600 years before Christ.

Daniel, who lived 600 years B. C. gives us in chapter iii. the following description. "This image's head was of *fine gold*—his breast and his arms of *silver*—his belly and his thighs of *brass*—his legs of *iron*—his feet part of *iron* and part of *clay*."

It consequently appears that the art of working the metals and precious stones, may be considered to have been understood at a very early period. The Jews and the Egyptians were well acquainted with metallurgy, as were also many other nations of the same period. The magnificent temple of Solomon was finished 1004 years B. C. To assist in the construction of it, Solomon sent for *Hiram* out of Tyre. Hiram must have been perfectly acquainted with his art, for we are told "he cast two pillars of *brass* of eighteen cubits high a piece, and a line of twelve cubits did compass either of them about."*

We find several important arts well understood soon after the deluge. "The art of spinning gold thread and interweaving it with stuffs—that of beating gold, and with light thin leaves of it to gild wood and other materials—the secret of casting metals, as brass, silver and gold—and of making all sorts of figures with them, in imitation of nature—of representing any kind of different objects—and of making an infinite variety of vessels of those metals for use and ornament."†

In the poems of Homer the metals and stones are frequently mentioned. M. Millin, in 1790, published a work on Homerian Mineralogy, which has been condensed and published in the Port-Folio of this city, 1814, by Professor Cooper. The Greeks and Trojans, at this early period, 900 B. C. were unquestionably acquainted with several of the metals, and the use and value of some of the stones—but I cannot extend my belief of their knowledge so far as either of the two gentlemen.

"Juno is said to have fixed in her ears, skilfully bored, ear rings, or three-eyed gems."

"Vulcan, in the Odyssey, makes a present of two dogs

* i. Kings, chap. vii. † Rollin's An. Hist. p. 185.

of gold and of silver to Alcinous, and images of young slaves as chandeliers."

The celebrated object of contention, the shield of Achilles, more valued for the workmanship than the material, goes far to prove the state of the arts at that period. The Greeks, as well as the Trojans, appear to have been familiar with *gold*, *silver*, *iron* and *copper*. Menelaus gave to Telemachus " a silver goblet with a golden rim." Thracian swords were in much request. Achilles made a prize of an iron ball sufficient to supply instruments of husbandry for five years. With the rust of the head of his lance of *copper*, he cured the wounds of Telephus. The shield of Agamemnon had "twenty bosses of white *tin* and twenty of *gold*, and one of the black *cyanus*." What is meant by cyanus we do not know—it is supposed to be lead.

The virtue of sulphur as a purifier was known to them. Ulysses purified his house with it after having killed the suitors of Penelope.

The properties of amber were also understood—a Phœnician merchant is said to have had "a golden necklace with beads of amber."

There is much reason to believe, nay there can scarcely be a doubt, that the Phœnicians were acquainted with the existence of the mines in England and Ireland, and that those islands are the same with the celebrated Cassiterides, from whence they procured their tin and carried on a secret and jealous commerce. These people were at a very early period possessed of much information, and were exceedingly intelligent. Some of their theories were so rational as to be maintained by many philosophers of the present time. "Ils pensoient qu'il y avoit eu successivement sur la terre des incendies et des déluges. Eusèbe rapporte leur doctrine d'apres Sanchoniaton. Ils croyoient que les terres et les mers avoient été enflammées—que des grands vents, des nuages succédèrent, qu'il tomba beaucoup d'eau. Cum igneum splendorem aer emisisset, ex ardenti maris ac terrarum inflammatione venti, nubes—extitere."*

* Delamétherie, Vol. 5, p. 352.

The extraction of lead or tin from its ore in the island of Cassiterides, according to several ancient authors, was discovered by Midacritus, but we are not informed in what year.

Xenophanes, founder of the Eleatic Sect, (535 B. C.) dwells upon the circumstance of petrified shells being found in the internal parts of mountains and in the bowels of the earth. He relates that the impressions of fish are to be found in the quarries of Syracuse and at Paros, and that an immense number of marine animals had been thus preserved, and inferred that those places must have been covered by the sea ages previously.* His opinions, however, on astronomy were wild and extravagant—he thought the stars were extinguished in the morning and rekindled at night.†

Among the early Grecian and Roman writers on natural history, there are many who treat more or less on mineralogy. *Herodotus*, the father of history, read his nine books at the Olympic Games 445 years B. C. In Clio, section 25, he relates that " a large *silver goblet*, with a *saucer of iron*, curiously inlaid," had been presented to the Oracle at Delphi. In the same book, section 215, he says the Massagetæ " make much use both of *gold* and *brass*. Their spears, their arrows and their battle axes are made of *brass*. Their helmets, their belts and their breast plates are decorated with *gold*." " They use neither *iron* nor *silver*, which indeed their country does not produce, though it abounds with *gold* and *brass*."

In Euterpe, section 4, the same author informs us, when speaking of Egypt, that " the name of their first king was *Menes*, in whose reign the whole of Egypt, except the province of Thebes, was one extended marsh. No part of that district which is now situate beyond the lake Mœris, was then to be seen; the distance between which lake and the sea is a journey of seven days." He also says, it is obvious to the inspection of common sagacity, that the port of

* Parkinson, p. 15. † Lempriere.

Egypt to which the Greeks then sailed, formerly constituted a part of the river. He tells us that Ethiopia produces *gold* in great quantities—that Cambyses sent to that country an *alabaster* box of perfumes, and that *gold dust* was found in great quantities in the river Pactolus, which flows through Lydia.

In the Encyclopædia Perthensis, article Mines, we are informed that " Lucretius says their discovery was made by the conflagration of woods, which melted the veins of metal in the earth beneath them:" and Aristotle tells us that some shepherds in Spain having set fire to the woods, the earth was thus heated to such a degree, that the silver near the surface of it melted and flowed into a mass, and that in a short time the metallic mass was discovered by the rending of the earth in the time of an earthquake. The same story is told by Strabo, who ascribes the discovery of the mines of Andalusia to this accident.

Aristotle, who wrote 384 years B. C. among the great number of his works, devoted some to mineralogy, metallurgy and the study of fossils. He attributed their generation to heat and cold, and to humidity and dryness—but he had neither division nor method.

Among the earliest attempts at a regular history and description of minerals is that of *Theophrastus*, in his history of stones, written about 300 years B. C. This illustrious Greek author was the north star of ancient mineralogists—and although we find him promulgating many monstrous errors, we are struck with astonishment at his comprehensive genius, when we reflect upon the crude state of natural science during the period he flourished. He taught that stones were formed by two general methods, by *afflux* and by *percolation*—evidently meaning aggregation and crystallization.*

The existence of petrifactions was noticed by *Erastosthenes*, *Strabo*, and other writers of this period†—but Theophrastus gives us a particular history of all the stones which were known at his time. He was acquainted with the ly-

* Theophrastus, paragraph 4.

† Parkinson, p. 15.

dian, or *touchstone*, which was used for the trial of metals—and describes the manufacture of *white lead* and *verdigrise*, as well as that of *glass*. He also notices *alabaster*, *the marbles*, *sardonyx*, *carbuncle* (which, he says, are engraved for seals), *sapphire*, *crysocolle*, *onyx*, *amethyst*, *prase*, *hematite*, *ochre*, *gypsum*, &c. &c. and many which are unknown at present.

The strange idea that stones were male and female was admitted by him, and received afterwards by Pliny and others. The attraction of amber (electrically) was observed by him—" but (he says) the greatest and most evident attractive quality is in that stone (magnetic iron) which attracts iron."* The escape of caloric from lime when moistened, attracted the attention and excited the astonishment of this author.

In the year seventy-nine died, perhaps the greatest of all the ancient naturalists, *Pliny* the elder. His curiosity to examine the eruption of Vesuvius in that year induced him to hazard his valuable life, which he lost by being suffocated with sulphureous matter.

Pliny enters largely into the description of earthy minerals, metals, the working of gems, &c.: but notwithstanding the great diligence he displayed, we find he scarcely advanced one step towards an accurate idea of the composition of minerals.

At an early period the universal custom of wearing rings, which were engraved and used as signatures to bonds, must have induced the search after the most perfect and choice specimens. Hence we find that every valuable gem in the modern systems was known and used by the ancients. To such an extent did they carry this branch of the fine arts, that modern lapidaries find it impossible to equal the exquisite workmanship of the ancients.

The sport of nature in representing various forms and figures in some of the precious stones, was observed and

* Theophrastus, paragraph 53.

taken advantage of by them. Pliny describes a stone which represented the nine Muses and Apollo very distinctly.

Factitious gems were constantly made, and frequently it was very difficult to distinguish them from the true ones. Pliny tells us he had works giving directions how to make all those things, but forbears to mention their authors, choosing rather to conceal deception than give currency to it—he afterwards gives the methods to discover all such cheats.

The *sardonyx** was valued above all other gems by Claudius Cæsar, and was so highly prized that it was imitated by cementing together plates of another mineral, black, white, and vermilion, so closely that it was impossible to discover the deception.

Many of the works of art of the ancients, which are still in existence, strike us with astonishment. The perfection of their statuary, of their buildings, of their cameos and intaglios, calls forth the greatest admiration. The height of the sphinx, which is cut from a solid rock and smoothly polished, is one hundred and forty-three feet. Some of the obelisks with a solid shaft measure two hundred feet.†

Pliny describes many fossil remains, the names of some of which are still retained—he speaks of *glossopetræ* like to a human tongue, which do not grow in the earth, but fall from heaven whilst the moon is in the wane. He also mentions the *Horn of Ammon*.‡

In his last book we have an attempt at a classification commencing with the *diamond*, which was rare at that time. With its qualities and uses he was well acquainted, but believed it to be entirely incombustible. His description of the *pearl* succeeded the diamond, and the *emerald* followed the pearl. This gem was much valued on account of its beautiful colour and its power of reflecting like a mirror. *Nero* used a fine one to enjoy the combats of his fencers,§ &c.—The fourth place was occupied by the *opal*, and the *ruby*, *topaz* and *jasper* followed in succession. To those are add-

* Pliny, book xxxvii. chap. 6.

† Pliny.

‡ Parkinson, p 16.

§ Pliny, book xxxvii. chap. 5.

ed a long alphabetical catalogue of minerals, generally unknown at the present time. The virtues of the various stones are carefully mentioned with each article.

To illustrate the state of mineralogical knowledge at this period, it will be necessary to mention some of the extraordinary and ridiculous virtues attributed to various stones.

We are told by Pliny, that *Milo* the celebrated wrestler, always carried with him some small stones taken from a cock's gizzard, to increase his strength and activity. The *blood-stone* was supposed to further the suit of any one carrying a petition to a king. Those who possessed Ammon's horn, it was supposed could see into futurity through their dreams. If an *agate* was held in the mouth, it was affirmed it would allay thirst. In Persia it was believed it would stop the rage of rivers and put storms to sleep. An agate of a simple colour was said to make a wrestler invincible. These idle stories, although mentioned by Pliny, were generally disbelieved by him—for he says, "those magicians will lie most monstrously."*

From this period until the sixteenth century, there were no naturalists who advanced the science of mineralogy. The same path was re-trodden by the followers of the various sects of ancient philosophers, with few or no deviations.—Literature and the sciences were making retrograde steps.

In the beginning of the sixteenth century the reformation gave a spring to inquiry and information. *Agricola*, a German, by some said to be a physician, by others a miner or agriculturist, published a work on minerals and metals, in two volumes folio, with figures well executed on wood.—He was the first who examined the external characters, and used them with judgment and accuracy. He defined their colours, hardness, softness, odour, figures, &c. His system divides minerals into those of homogeneous and those of heterogeneous parts. The first he "subdivides into four classes—1st. terra—2d. succus concretus—3d. lapis—and 4th.

* Book xxxvii.

metallum. The last he divided into compound and mixed minerals."*

About this time *Gesner*, called the German Pliny, gave a learned work to the world on fossils, minerals, &c. which has been much esteemed by men of science.

Jean Bauhin, a very distinguished physician, born at Amies, in this century published his researches. In his system of minerals he commenced with the least and finished with the most perfect. He described the *Belemnites*, *Astroites*, &c. and many under the same names that are now generally received.

The celebrated *Aldrovandus*, professor of philosophy and medicine at Bologne, died in 1605, and left his manuscripts and collection to that city. He appears to have surpassed his predecessors in most of the branches of natural science. His works form thirteen volümes folio, with plates—three on *ornithology*—one on *insects*—one on *shells*—one on *serpents*—three on *quadrupeds*—one on *fish*—one on *monsters*—one on *trees*—and one on *mineralogy*. These were considered the best works on the subject of natural history that had been written.†

At an early period of the seventeenth century science had many able supporters. At this period *Anselme Boot*, of Bruges, gave his treatise on *stones* and *jewels*—but its principal merit appears to be that of possessing some kind of order of system.

The great philosopher *Kircher*, soon after this produced many excellent works. His "Mundus Subterraneus" was in folio, with figures. In this he describes the earthquake at Calabria and the eruption of Vesuvius, descriptions which are still read with delight. He wrote on the admirable construction of the universe—on the centre of gravity—on volcanoes—on rivers, winds, fossils, minerals, animals, &c. He opposed most strongly the idea of alchymy and the philosopher's stone and its properties.‡

* Jameson, Parkinson, and D'Argenville.

† D'Argenville, p. 14.

‡ D'Argenville.

Parkinson informs us, (p. 20) that in the seventeenth century the collection of fossils became much more general. Only two catalogues of collections had hitherto appeared—but now several very extensive ones were published. In 1622 appeared a very copious description of the celebrated museum of *Calceolarius* of Verona—and twenty years after that, was published the catalogue of *Besler's* collection. In 1652 appeared *Wormius's* catalogue—in 1663 was published *Spencer's*, and in 1666 *Septala's*. An account of the museum of the king of Denmark was published in 1669—in 1674 *Cottorp's* catalogue appeared—and in 1687 was published that of the celebrated *Kircher*—in 1687 *Dr. Green* wrote an account of the curiosities which were contained in the museum of Gresham College—and in 1695 appeared the catalogue of *Petiver*, an apothecary of London, who at a vast expense had formed a most valuable collection.

Several writers of considerable talent in this century are unnoticed by Parkinson. *Lazare Erckern*, inspector of mines to his imperial majesty of Germany. *Etienne de Clave*, a physician, combated in 1635 the opinions of *Aristotle*, *Theophrastus*, *Agricola* and others, on the causes and composition of stones. *Alvarez Barba*, a Spaniard, curé of St. Bernard in Peru, had the reputation of being a great metallurgist. In 1640 he gave the world a great work, "Arte de los metales," in one hundred and thirty-nine chapters, on metals and minerals.*

Jean Jonston, a Dutch physician, in 1653 published a fine work in folio, with plates engraved on copper.

His system is curious in being divided into colours, which is evidently very futile. His first division was diaphanous, which was again divided into white, red, blue, green, yellow, black, and those of various colours, as the opals—second, into semi-diaphanous and semi-opaque—third, into opaque stones, (meaning rocks)—fourth, stones of a particular figure—fifth, of doubtful stones—sixth, those taken from animals.† To the list of writers of reputation who flourished at that

* D'Argenville.——† Ibid.

period, we must add *Jean Jacob Scheuchzer*, *Nicholas Lemery*, *Bounani*, and *Edward Luidius*.

The eighteenth century at an early period produced two men of great industry and talent, who gave the result of their researches in natural history. *Leuwenhoek*, so well known for his microscopic discoveries, published his great work, 4 vols. 4to. in 1719, in which he describes the objects of almost every branch of natural history. But *John Woodward*, in his work entitled " An attempt towards a Natural History of the Fossils of England in 1729," did more perhaps for the science than any of his predecessors. He suggested a new method to arrange minerals, and was universally respected as a great naturalist.*

Francis Colonne, a physician and chemist of great eminence, about this time wrote on the natural history of the universe. He entertained the curious idea that mountains grew, and that nothing was easier than to make gold, for which he gave a recipe.

The great theologian, *Swedenborg*, in 1734 published some volumes on philosophy—he treats of fossils, minerals and metallurgy, on a very extensive scale.† In the following year, *Albert Seba* gave to the world his large work in folio, on most of the branches of natural history. It is said he possessed a fine cabinet—but unfortunately he did not live to publish his book on fossils and minerals as he designed.

The academy is possessed of a copy of his works as far as printed.

Parkinson (p. 25) observes, that " the eighteenth century commenced under the most favourable circumstances for this science," and we may add to the list of authors already spoken of, the names of *Mylius*, *Bajer*, *Rosinus*, *Buckman*, *Linck*, *Jacobus a Melle*, *Harenberg*, *Reamur*, and other men of learning.

True philosophy now began to shed its light over the dark mazes that had entangled science—the development

* D'Argenville.—† Ibid.

of the laws of nature chased from the pages of history the monstrous and unintelligible stories of the last century. It could no longer be believed that a stone, shaped like a human thigh, as related by *Eusebius*, in Holland, would find its way back at night to a particular place, however far it might have been carried during the day, even though fastened by strong iron chains to another stone. Previous to this period were supposed to exist petrified birds—some with spread, others with closed wings—also, bees and wasps resting on their curiously worked cells—and spiders weaving webs—moths and butterflies engendering, &c.*

The great father of systems, the illustrious *Linnæus*, in 1736 published the first sketch of his mineralogical system.† Six years previously, *Boerhaave* also published a system, but this great physician himself could not withstand the splendour of our naturalist. The intelligent and penetrating mind of Linnæus perceived the great importance of a proper system for the attainment of natural science.

The encyclopedists inform us, that "it is chiefly from this period that the origin of systematic mineralogy may be dated, when our science, with other branches of natural history, acquired a degree of popularity which it had never enjoyed before."‡

Although Linnæus's mineralogical system at the present time, cannot be compared with those of his botany and zoology, we can truly say it is possessed of great merit, when we reflect on the almost total want of chemical analysis which is so essential to a complete system of this science. He divided minerals into first, *petra*—second, *mineralia*—third, *fossilia*. Those three he subdivided into three each. *Petra* into vitrescentes, calcaria, apyræ—*mineralia* into salia, sulphuria and mercurialia—*fossilia* into concretæ, petrifactæ and terræ.

Under the impression that the crystalline form of minerals was due to different salts, he classed them accordingly. This was an error—but it in some measure laid the foundation of

* Parkinson, p. 26. † Jameson. ‡ Dr. Rees's Mineralogy.

crystallography—it induced men of science to examine into the structure of minerals with great success. He gave three editions of his Systema Naturæ, which Delametherie says he so much improved as to become one of the best books we have.*

About this time *Pott* of Berlin, and *Henkle*, made very considerable improvements in the science. Pott's great chemical knowledge and industrious application, enabled him to make many valuable discoveries—his knowledge of analysis afforded him an opportunity of giving an arrangement that would do him credit at the present day. Unfortunately he died previously to his being enabled to analyze the metals.†

Wallerius, in 1749 published at Stockholm his system of mineralogy. Being ignorant of the improvements of Pott, he fell in a great measure into the old system—uniformity and exactness of description were however attained by him, which added much facility to the acquirement of the science. His arrangement nevertheless was very incongruous. The characters of the first, second and fourth classes, were derived from their external form—those of the third, from the manner in which the minerals contained other substances.‡

D'Argenville published in 1755 his "Histoire Naturelle," in which he treats of the "earth, stones, metals, minerals and other fossils," as he expresses it. At this period the systems were still, comparatively, in a very crude and indigested state. D'Argenville was very deservedly held in much respect for his scientific researches—but when the eye accustomed to the present perfect and condensed systems, wanders over this author's, it is ready to close with disgust and turn forever from the science. His "new method," as he calls it, is given in Latin and French, in double columns, and fills fifty-five quarto pages. He frightens us with the never ending extent of his subdivisions. The first class, *terræ*, is divided into *terra melitensis*, *terra persica*, &c. &c. to the number of forty-three. The marbles are divided into nearly

* Vol. I. p. 13. † Rees's Cyclopœdia.—‡ Ibid.

two hundred and fifty different kinds. In fact every locality with him appears to entitle the marble to a name and a place in his classification, and thus with other substances.

In the class of fossils proper, we have animal remains subdivided into about three hundred and fifty species—in truth every specimen seems to have a place in his system, and we may have some idea of the heterogeneous mass it constitutes, when we see the first five of this part of the catalogue, viz:

1. "Part of a human skeleton in slate.
2. Skeleton of a man reported by Kircher.
3. Fœtus petrified in the belly.
4. Another fœtus petrified in the abdomen.
5. Skull of a man petrified, &c. &c."

I shall now proceed to say something on the body of his work. On the subject of *rock crystal*, he seriously informs us it is not congealed water, for if so it would be consumed by fire. His ideas on calcarious rocks are however just, and he very properly ridicules the idea of *Tournefort*, that "stones and marbles have a germ and vegetate like plants." D'Argenville supposed the "minerals and metals," to have the same principles, and that it might be said that metals were refined minerals.*

Notwithstanding the great advances that had been made at this period in science, we find many of the most erroneous and ridiculous ideas still prevailing. The *eagle stone*, (Ætites,) of which *Gesner* admitted fifteen species,† and which is not admitted, and very justly, at this time in the systems even *as a variety*, was supposed to possess very peculiar virtues in assisting parturition, and for this purpose it was said the eagle carried it to her nest.

Without exactly denying the fact, *D'Argenville* passes over Kircher's relation of a whole city in Africa, with its inhabitants, being petrified—and that a horde of Tartars had undergone the same wonderful change!!‡ He has given us the figure of a *priapolite*, which displays the penis and

* P. 277. † P. 302. ‡ P. 329.

testes, evidently the accidental form of the deposition of carbonate of lime, a stalactite. From an ideal resemblance of some of the geodes to the testes, he divides them into varieties according to their number—thus a single one is called *orchis*—a double one diorchis—a treble one triorchis, &c.—*Diphis* represents the two natures of male and female. Thus it would appear every new form of every mineral substance would require a new name. Should we follow this system, our catalogues would swell to volumes without any adequate benefit, and the mass of matter to be waded through would deter the student from attempting the threshold.

In 1758 the celebrated analytic chemist, *Cronstedt*, gave to the world his system of mineralogy. It has been observed that with him began the second principal era in mineralogy, if *Agricola's* be considered the first. Certain it is, he excelled all that had gone before him. He not only paid strict attention to the chemical composition of minerals so far as he was able, but he examined into the effect of the atmosphere on them in their natural state.

He divided them into earths, salts, bitumens and metals. The first class he subdivided into nine orders, calcareæ, siliceæ, granatinæ, argillaceæ, micaceæ, fluores, asbestinæ, zeolithicæ and magnesiæ—the second into acida and alkalina—the third into one order—the fourth into perfecta and semi-metalla.* It is easy in the present advanced state of the science to discover many errors in this system, but when we reflect upon the numerous difficulties to be overcome at the period when Cronstedt wrote, we are astonished at the advances he made towards a perfect system. His principles of classification are adhered to by most chemical mineralogists of the present day. Cronstedt was the discoverer of *nickel*, and the first to class the new metal *platina.*†

About this time a new acid was discovered in the fluate of lime, a new earth, magnesia, in talc and asbestus—and

* Jameson, p. 6. † Rees's Cyclopædia.

new metals in manganese and wolfram. Cronstedt rejected many substances admitted by his predecessors, particularly those that had been received on account of their peculiar forms only. We are told that no work on mineralogy ever created a greater sensation than that of Cronstedt.* It was soon translated into almost all the European languages, and was adopted by almost all writers on the subject. Rocks and fossils were by him first separated from mineralogy, in which he certainly did the science an important service. His system was throughout chemical, and it may be said to be a fault that he paid too little attention to external characters.†

A few years after this period, *Engstroom* pointed out the method of subjecting mineral substances to the test of the blowpipe, and added much to the facility of obtaining their composition.

The student of crystallography owes much to the labours of the ingenious and intelligent Romé de L'Isle, who published his excellent work on that subject in 1773. He is styled the father of crystallography, and is celebrated as well for the elegance as the accuracy of his description of crystalline forms, for which he stood unrivalled at his time. His work is considered a chef-d'œuvre. To him *M. D'Agoty*, who published his splendid work on crystals, with coloured plates, in 1781, owes the greatest obligations.

Sir John Hill in 1771, published his arrangement of minerals. We may form some idea of what might be expected when we are told in the first paragraph of the introduction, that the arrangement is founded on their obvious characters, "*without the skill of chemistry or fatigue of experiments—without furnaces or aqua fortis.*"

The first class is *talc*, genus *isinglass*. The members of this genus are *Muscovy isinglass*, *Danish isinglass*, *Indian isinglass*, *Persian isinglass*, *French isinglass*, *Iceland isinglass*, *Hungarian isinglass*, and *orpiment insinglass*. Genus third of the same class is *black lead!* and this he divides

* Dr. Rees's Cyclopœdia. † Jameson.

into *pure black lead, scaly black lead, plated black lead, dusty black lead, perplexed black lead, radiated black lead, kidney black lead, dingy black lead, and bright black lead.*

In this manner is the catalogue increased with the description to an octavo volume. To prove the utter fallacy of this system, it is only necessary to show that we could bring in the same pieces of *quartz* under twenty-five different names, viz: "*pure crystal, white crystal, pure sprig crystal, broad sprig crystal, narrow sprig crystal, long sprig crystal, short sprig crystal, clear double crystal, low crystal, coated crystal, water crystal, hollow crystal, pure crystalline sand, round crystalline sand, volatile sand, hill sand, coated sand, heath sand, rugged sand, pebble crystal, white pebble crystal, sandy pebble crystal, shattery pebble crystal, black crystal, curtain crystal.*"

Who can look at this system without feeling at a loss how to account for so much folly entering the brain of man? What a contrast does it form to *Werner's* simple and excellent arrangement? This father of modern mineralogy published his system at Leipzig in 1774. His work constitutes a new era in the science. By his simple and condensed arrangement, he offered results to the student which could not be obtained from the massive systems that had preceded him. He studied minutely the external characters of minerals, and gave them so much order that his system may be said to have enticed while it instructed. He also paid strict attention to the essential component parts of minerals. His classification is so generally studied and known, that I forbear to enlarge upon it. *Kirwan* the Irish naturalist first made this system known in England,* and by his unremitted labours added much to the general stock of knowledge.

In 1797, *Delametherie*, celebrated for his "Theory of the Earth," published his excellent and accurate system of mineralogy. The great *Haüy* in 1801, first published his excellent work on crystallography, in which he certainly sur-

* Dr. Rees's Cyclopœdia.

passes all authors that had previously written on the subject—detur dignissimo.

Parkinson published his work on fossils in 1804, which, although not strictly a scientific production, deserves much praise, particularly on account of its beautiful coloured plates. The great industry of the author in searching out specimens and giving exact representations of them, justly entitled him to the thanks of the scientific world.

The works of the analyzers and writers on mineralogy of merit within the last thirty years, are so well known and appreciated, I pass them over with mentioning the names of *Klaproth*, *Brochant*, *Brogniart*, *Berzelius*, *Phillips*, *Jameson* and *Vauquelin*, all of whom have published with much credit to themselves and advantage to the world.

With regard to the state of our science in other countries than those reviewed, we know but little, and what we do know is unfavourable and discouraging.

Chantreau in his travels through Russia, informs us that *Demidoff*, a merchant of Moscow, discovered in 1725 in the mountains of Koliwan a mine of copper. The College of Mines, where his specimens were exhibited, gave him a patent for a *copper* mine. Demidoff extracted from his mineral two fifths of silver, and continued to work the mine in Siberia for twenty years. At the end of this period, being sufficiently rich, he gave up the mine, and paid as an indemnity for using it as a silver mine, three thousand rubles rent. These mines occupied nearly forty thousand men about the year 1790.

Pallas, who was born and educated at Berlin, was called by Catherine II. to Petersburg in 1767. His character, as a traveller and great naturalist, is well known—he was employed six years in examining Russia and Siberia, and threw much light on their natural history.

Morier, in his travels in 1808 and 9, informs us that the Persians had discovered mines of "iron and brass," which were worked by their own ingenuity and answered perfectly well.

If we now take a retrospective view and observe the con-

trast between the early dark ages and the present enlightened one, we have much reason to congratulate ourselves upon having the advantage of the learned works of the present. But our enjoyment is tempered with regret to find the lights of science have not yet spread more generally throughout the world. Superstition yet has its influence in places that should have long since thrown off its hideous yoke. *Sonini* tells us that the inhabitants of Malta and Gozzo, believe the glossopetræ and bufonites to be the tongues and eyes of serpents miraculously destroyed by St. Paul.

When we turn to our own country, we have peculiar reasons for gratification. The enlightened form of our government—our numerous liberal institutions for the instruction of youth—the easy access to collections and the facility of making them, have done much to promote the easy acquirement of this as well as other sciences. Within the last fifteen years, many valuable collections have been formed in the United States, some at a very considerable expense. Many works on the subject have been reprinted here, and some have been published that are not exotic. It is with peculiar pleasure we mention the work of professor Cleaveland. He has given us a mass of useful information that can be found in no other single work on the science. His classification is "chemical, as strictly as the present state of mineralogical knowledge will permit,"* and on the whole we think it may be said, in truth, that it is the best elementary work we are acquainted with on this subject.

The name of William M'Clure should be recorded here as the most efficient promoter of mineralogical knowledge in this country. His numerous and valuable donations of rare minerals, geological specimens and books to our public as well as private collections, entitle him to our warmest acknowledgments. His work on the Geology of the United States is too well known to be expatiated on here.

The aborigines of America appear to have been almost entirely ignorant of the metals. "All the savage tribes,

* Cleaveland, p. 97.

scattered over the continent and islands, were totally unacquainted with the metals which their soil produces in great abundance, if we except some trifling quantity of gold which they picked up in the torrents that descended from their mountains, and formed into ornaments."*

This forms a deep contrast to the present flourishing state of our knowledge of minerals. In this city alone there are at least twenty good collections, and many others in an incipient state. Perhaps in no part of the world has the science been more assiduously cultivated than in the United States.

The study of mineralogy, with all the other sciences, presents great difficulties to arrive at some of its phenomena, and in such cases we must agree with Bergman, when he says, "I had rather, with Newton, ingenuously confess my ignorance and know few things, but these certain and determinate, than with Des Cartes, explain every thing upon forged or false principles." In the investigation of nature we should search for truths alone—"bene scire, est per causas scire."

TO DR. A. WILSON PHILIP.

Dear Sir—The advantage which I have derived from the perusal of various productions of your eloquent pen, and the profound respect which I entertain for your genius, industry, and candour, manifested in a zealous devotion to the promotion of medical science, must be my apology for requesting your attention to the following pages, in which I have attempted to extend your doctrine of inflammation to the explanation of those parallel phenomena which are observable in fevers.

I am, dear Sir,

Very truly yours, &c.

C. D. MEIGS.

* Robertson, p. 185.

ART. VI. *Lecture on the State of the Blood-Vessels in Fevers.* Read before the Philadelphia Medical Society, January 17th, 1824. By C. D. MEIGS, M. D.

BEFORE we enter fully into the consideration of the subject to be examined, let us come to an uniform understanding and interpretation of one particular word, without a conventional acceptation of which I fear we may waste our time in idle quibbling, instead of drawing from our evening occupation some useful hints on some more available information.

The word to which I allude is action: what do we mean when we say action? Have any three gentlemen present the same ideas of its meaning? Doubtless those who have read the article in Parr's Medical Dictionary, a work of high rank and authority in our science, will have remarked that it means any thing—and that if the compiler of a dictionary, who is supposed to be more precisely correct, ex professo, than another man, does not give to a word an exact valuation, it is time that we come to some agreement about it.

The word action, as occurring every where in the medical books, is as vague as it is in Parr's work—e. g. in Cullen, M'Bride, Fordyce, and even in the writings of W. Philip—the last of whom ought to have been especially careful in the application of it, considering the peculiar views he entertained concerning the state of the vessels in inflammation.

The article Action in the Dictionnaire des Sciences Médicales is the best I am acquainted with. It defines action in general as mouvement, on suite de mouvemens, dirigèes, vers un but determinè, and recognises four sorts of action—viz. 1. chemical action—2. physical action—3. physiological action—4. moral action. It is only with the third sort, or physiological action, that we have any concern here: of this the Dictionnaire des Sciences Médicales says, enfin, l'action physiologique est encore mouvement, mais mouvement, qui s'execute dans un etre vivant, et par l'effet des forces vitales; c'est ainsi qu'on dit, l'action d'un muscle,

l'action de l'estomac; les actions de cè dernier genre qui sont un peu compliquèes prennent le nom de fonctions.

Even this account is not entirely satisfactory—for though it points out the distinction between action and function, it still recognises them as convertible terms.

I will paraphrase the French passage in the following manner:—Physiological action is vital motion—and by this word action we mean to express our idea of motion in any single part, as thus—a muscle moves, a muscle contracts, a muscle acts, the action of a muscle: an idea perfectly simple, being only the idea of approximation of, or the effort to approximate, two extremities of a given fibre or muscle.—Or we mean by it (*action*) to express our ideas of a compound movement or series of movements, as when we say the stomach digests, or the stomach acts, the action of the stomach—and this latter use of the term comprises the ideas of contraction of the muscular fibres of the stomach, augmentation of its secretory phenomena, or its insensible organic contractility, chemical action in the solvent operation of the gastric juice, saliva, &c. and some indefinite conjectures about the influence of the nervous power, in so modifying chemical action as to produce an animal result called chyle. The latter mode of using the word action, is evidently therefore improper and vague—the proper term is function.

I will give one example of the proper application of our word. Action of the bladder. The action of the bladder is mere contraction, mere exercise, of what Haller called irritability. The action of the bladder is to expel the urine—the function of the bladder is to contain the urine.

When therefore I use the term action, in relating a supposed physiological or pathological condition of the heart, of an artery, or a vein, I do not use it to express the whole of that great vital function the circulation of the blood, but only as hypothetic of the state of a single fibre, or all the fibres of the heart, of an artery, or vein, or all the arteries, or all the veins. If I say action I mean action—if I say function I mean the function. Now as it will not be easy to misap-

prehend my use of this vague term, I proceed to the business of my lecture.

The degree of vitality in a healthy robust man, is higher than in one worn out with disease, and on the point of parting with all the properties which distinguish him as a living being.

The strength of a muscle is the exponent of its degree of vitality.

In proportion as any muscle is stronger in a physiological sense, so will its action be stronger—its contraction more easily and perfectly effected.

In the cold stage of fever the arteries contract with more force than in the hot stage—for force is only a relative term, as heat is only a relative term.

They contract with less force in the hot stage.

They are in a state intermediate of the two former in the sweating stage.

I believe the truth of a celebrated proposition of Vacca, defended by Dr. Lubbock and Mr. Allen, fully illustrated by Dr. W. Philip and admitted by Dr. Thompson--viz. that in inflammation the capillary vessels are dilated and debilitated.

I believe that fever is the archetype of inflammation—fever being in the whole system what inflammation is in the capillary vessels.

I think that a dilated and debilitated condition of capillaries being taken in evidence, and as explanation of the phenomena of inflammation, a similar dilated condition is to be taken as evidence of an analogous state of arteries in fever—for fever is the archetype of inflammation, and a certain pathology being admitted in one case is established in the other.

I am so well satisfied with the truth and reasonableness of the doctrine of inflammation set forth by W. Philip, that I can hardly imagine any one here present so unaffected by his reasoning as not to be "almost persuaded," and as his writings are so much read and known in this coun-

try that his arguments are become common and trite, I shall not go over them here.

The science of medicine is much indebted to the brilliant and analytical genius of Bichat, for the happy division, investigation and present arrangement of what he called the system of the body, by which we are enabled to appreciate the different degrees of vitality of the various constituent tissues of the body—and by which also we find, as for example in the mucous tissue, a great uniformity both of structure, properties and application, from the knowledge of which we may draw the most important practical lessons. So also in the muscular, serous, &c. &c. These are all governed by particular laws which pervade every part of them—the law of one part being the law of every part of the same kind.

Wherefore shall we not admit the same unity of properties in the circulating system? Have we not in our pathological reasonings lost sight of the advantages to be derived from Bichat's arrangements, and separated too widely our ideas of the venous tissues from those which we have of the arterial and capillary, and all of them, to an infinite distance from our ordinary notions of the heart?

The heart and vessels constitute one single system. The heart is part of the circulating tissues, and are we to regard it as a mere engine, a forcing pump placed in our breasts to urge on the current of blood, possessing no sympathies with, having no feeling of relation to, the vessels of which it is a continuous and subservient portion?

On the contrary, it is an essential portion of that system of tissues to which it is attached and subservient—enjoying the same *kind* of vitality—dependent on the same sort of (ganglionic) nerves—therefore governed by the same laws, susceptible of the same exaltations and diminutions of action with them. Whence I infer that increased action of the heart, (as a general proposition) argues the same condition of the vessels, and v.v. Thus, if my heart be so excitable and excited as to resist more than is natural a full

dilatation of its ventricles, the vessels will partake of the same pathological condition (generally.)

I would not have you suppose me ignorant of many circumstances of difference in different portions of the vascular system. Bichat has pointed them out—but they cannot prevent me from considering the whole system as an unit of tissues.

I know that the veins and arteries are each isolated by the intervention of two capillary systems—that the veins are the receivers of every thing that enters into our intimate structure—that they constitute a great reservoir or cistern of all the fluids, holding all the products of both lacteal and lymphatic absorption, besides all the blood deprived of its arterial properties in the previous circulation. They contain more blood than the arteries.

"Lumen reliquarum venarum (he excepts the pulmonary) ubique lumine arteriarum majus est; contenta vero uti lumina sunt, cum longitudines utrinque pares sint." Haller Phy. tom. I. p. 131.

They circulate it more slowly, and depend in *some measure* on extrinsic causes for the exercise of their function.

They are much more distensible than arteries—"facilius enim cedunt et majus dilatantur arteriis, non solum certe ratione quadrupla sed longe majori."—Haller: and hence they are not so strong—and when overburthened or distended get rid of the load slowly and difficultly.

The arteries on the contrary receive nothing except from the veins. They expend every thing to the amount of six or eight pounds per day—they have, besides the office of holding and circulating the blood, the much more difficult duty of furnishing to, and probably of executing the multifarious operations of exhalation, secretion and accretion—which evidently gives them scope for a wider and more extended relation with the actions of other parts both in health and disease.

The former is a careful usurer who hoards up,

"quicquid verritur"—

the latter is a reckless prodigal, who squanders on the sys-

tems of tissues the profusion of abundance and plenteousness which the former had painfully gathered together—and to pursue the figure, the veins have an irritability of a less sudden, impetuous and fickle character—they are steady, exact, uniform and methodical of their own accord, but the arteries are liable to sudden derangements of action and temper from slight causes—they are eminently fickle and variable. An emotion will cause them to blush, and the slightest surprise will make them pale.

The pathological state of the veins is I think almost always referrible to a condition primary in the arteries.

Both veins and arteries are constantly exercising a power antagonist to that of the heart.

In health they antagonise it perfectly—in disease imperfectly—either too much or too little.

They of course antagonise each other. Both possess the power of diminishing their diameters, the length of which depends on the degree of an antagonist force.

Therefore when they are small it is because they antagonise with more force, more action—when large, because they do so with less force, less action.

But they are small in a chill, and large in a fever.

What do we see then in the large, round, full pulse of fever, except the proofs of diminished resistance to antagonist power, and consequently the proofs of at least a relative debility.

The tendency of arterial and venous action (contraction) is to diminish the respective tubes—but if the arterial action, in consequence of a superior contractility, be greater in any given example than that of the veins, or if the resistance of the veins becomes less than that of the arteries from any cause, it follows that the arteries will become morbidly small and hold a smaller quantity of blood, and the surplus of that incompressible fluid which they exclude is accumulated, where? in the weaker, less resisting tubes, the veins.

Here you see plainly, that increased action of the arterial vessels have an uniform and unquestionable tendency to

destroy the balance between the two systems, of red and black blood.

No physiological action of animal or organic life can be continued in a preternatural degree of force for any considerable duration, without inducing debility in the part thus acting. This is a prevalent law of the whole animal creation.

If I bear a great weight ten minutes, I shall be less able to sustain it other ten.

If my heart and arteries are in a state of increased action this forenooon, they will generally be less in action this afternoon.

If by their inordinate force they have thrown a considerable surplus of blood into the veins this forenoon, then these same veins by exhaustion of power of their antagonists, or by other causes, will be placed in a condition of equivalent or superior action this afternoon, for exertion is followed by exhaustion. Hence if my arteries have caused a venous congestion—or in other words, if I have a very small frequent pulse—or in other words, if I have a chill this forenoon, I shall have a fever and sweat this afternoon—for the veins will be stronger and the arteries weaker: and from the foregoing alone, I can deduce very justly and legitimately, the doctrine which I am upholding.

I propose this doctrine, because any other is actually unintelligible and inapplicable and incongruous. Examine for example that of Dr. Cullen, who says, xlvi. "our doctrine of fever is explicitly this: The remote causes are certain sedative powers applied to the nervous system, which by diminishing the energy of the brain thereby produce a debility in the whole of the functions—and particularly in the function (action?) of the extreme vessels—such however is at the same time the nature of the animal economy, that this debility proves an indirect stimulus to the sanguiferous system—whence by the intervention of the cold stage and spasm connected with it, the action of the heart and arteries is increased and continues so, till it has had the effect of restoring the energy of the brain, of extending this energy to the extreme vessels, of restoring therefore their action,

and thereby especially overcoming the spasm affecting them, upon the removing of which the excretion of sweat and other marks of the relaxation of capillaries takes place."

This is the Cullenian theory summed up explicitly. It has ranked great names under its banner, and nevertheless it seems to me impossible, erroneous, unintelligible.

It assumes that marsh miasmata are sedatives—that they act on the nervous system to weaken it, and indirectly stimulate the vessels to overcome by strong action a strong contraction of capillaries caused by and called weakness, upon the removal of which strong action, the strength returns to them, as evinced by marks of their relaxation.

Can any one understand it? Certainly no one. Let us now get through the remainder of our subject, which at the risk of being misunderstood must be done briefly.

One part of a system of tissues may be weaker than another, but this is a morbid condition not opposed to my former proposition.

One part of the venous portion of the vascular system may be weaker than another.

Probably the greater trunks whose power is said to diminish in proportion as the size increases, are the principal seats of this relative weakness, and that they therefore become the seats of venous congestions or engorgements. This is the case in some of the cetaceous and web-footed animals. Blumenbach says that the common and sea otter and the dolphin, have a peculiar tortuous arrangement of their great venous trunk, for the very purpose of permitting a safe congestion on the right side of the heart while the animal is unable to breathe under water.

I could cite a thousand passages of respectable writers to prove the existence of venous congestions—but I shall not, for the observation is palpably and demonstrably true—but they wish to explain it by the incomprehensible argument of debility—diminished action of the heart and arteries. But if now without further illustrations, you are willing to admit, that increased action of the heart and arteries tends to diminish their contents and pile them up in

the veins, you immediately perceive that when they act most powerfully, when they antagonize most powerfully, when their calibre is smallest (" uti lumina ita contenta,") we shall have a shrinking of the surface of the body, with paleness, coldness, cutis anserina, a small frequent pulse at the wrist, and wherever we can come at an artery to feel it, we shall have those symptoms which denote fulness of the venous trunks or accumulation on the right side of the heart, as gaping, sighing, anxiety, præcordial oppression, indications of a difficult pulmonary circulation and function, nausea, vomiting, horrors, cold extremities with a hot centre.

Is this delineation of the cold stage of a fever in keeping with the foregoing arguments and doctrine?

I said above, " frequent pulse," the pulse according to my observation, is almost uniformly frequent. I counted the pulse of a woman whose ague commenced ten minutes before, it was small and ninety-four—in five minutes one hundred and two—five minutes one hundred and twelve—five minutes one hundred and fourteen and the teeth chattering together—her pulse went on increasing in frequency in proportion as the ague was more intense.

In another case the pulse in a violent ague was very small and one hundred and six.

Any man of observation will know, that in a tertian which shall attack his patient at ten A. M. the pulse at seven, eight or nine A. M. is already preternaturally frequent, and sometimes even of considerable volume and hardness—it is only as the action of the arteries increases predominantly over that of the veins, that the pulse of chill becomes smaller and the horrors and other phenomena of that stage take place. But to proceed—

If the arteries in consequence of their increased action should have their energy reduced by exhaustion to a level with that of the veins, or should the veins by the stimulus of distention recover their superiority, or in any other manner, we shall next perceive the evidences of their reaction, i. e. we shall have striction or contraction of the veins, with parallel diminution of their capacity and contents—for " uti

lumina ita contenta." The effect of this is seen in the other parts of the vascular system, by return of warmth and colour to the surface—the heart less irritated, less in action, allows its fibres to be completely distended and its cavities completely filled—it takes in its full two ounces and a half of blood, which when it is thrown out into the vessels, produces a large round full pulse, a cessation of chills, a red, turgid, plump, smooth, hot skin, in place of the cold, shrunken, rough anserinous skin of the cold stage. The temperature is equable—we have no more gaping, sighing, &c.—we have acute pain of the head instead of the dull heavy one, intolerance of light, tinnitus aurium, throbbing temples, vigilance, delirium. Such is the hot stage. Is it not easily comprehended, that it will advance pari passu with increased resistance of the veins and relaxation of the action, or relative debility of their antagonists?

But what is the third stage? I answer, that the natural termination of the foregoing condition, is to be looked for in some evacuation which, by diminishing the quantum of fluids and removing irritating recrementitial particles retained in the two former, may reduce the mass and momentum and stimulus. The momentum of the circulation is now very great, for the arterial, capillary and venous system, and the heart, are now equipoise—the whole system is pervious, being equally free for the passage of blood, with a heart beating oftener and throwing out more blood than in a state of health.

If this be true, the following passage from Boerhaave's Institutes, p. 382, is incorrect: "à fibris irritatis and sanguine celerius per aperta acto quia venis revebitur sed arteriis in multis prohibetur acceleratur pulsus, fit febris, sitis, calor, vigilia, debilitas, molestia."

If the evacuation above spoken of, be happily effected by hemorrhage spontaneous on artificial, by sweat, urine or stool, we shall have what Hippocrates called a κρισις, a judgment, decree, decision, termination of the morbid contest of action—all parts of the vascular system subsiding alike and justly to their balanced and natural proportion of

action and function. But if the veins now sink by exhaustion of power below their comparative natural grade, and the arteries in this manner, or by the reapplication of the morbific cause, acquire the superiority of force, we must have a repetition of the paroxysm—remittent, quotidian, tertian, or in any other type, and this again and again, till some new and more perfect crisis restores the balances of the sanguiferous system, or till death is the consequence of these morbid derangements of our most important and indispensable vital function, the circulation of the blood.

Such as are not bound to consider every doctrine not laid down in the written code, as an idle and useless or pernicious innovation, are requested to examine this one carefully. I most earnestly recommend to them the writings of that eminent physiologist, W. Philip, who, by satisfactorily elucidating the state of the vessels in inflammation, has principally led me to the adoption of the foregoing theory of their state in fever—a doctrine which he has almost published in his work on febrile diseases, and in a paper in the Edinburgh Medical and Surgical Journal, vol. ix. p. 435. Let such persons take with them this honest passage from that most profound, candid, and philosophical physician, baron Haller: "Monemur ne quidquam ideo pro vero accipiamus quia recepta est, sed experimenta acquiramus, quæ, fidem nostris opinionibus faciant"—a sentiment which, as it was eminently the rule of his conduct in philosophizing, will be of inestimable value if it causes one of us to resemble him even in a remote degree.

Do you ask me what advantage would result from a general reception of this doctrine. I answer that I believe it a true one: and that truth is valuable as mere truth: and also, that our reasonings are made up of our comparisons, our judgments of our reasonings, and our practice should always be the result of our judgments.

What does this doctrine teach me? It teaches me that I should commit a murder by bleeding a patient in a violent attack of chill or ague, because I should, knowingly or voluntarily, increase a disturbance in the circulation, of itself often sufficient to extinguish the powers of life.

It teaches me when and why I should bleed in fever.

It teaches me not to give brandy and red pepper in pleurisy, nor bark in acute rheumatism. It tends, I humbly hope, to make me a disciple of Hippocrates, that humble servant of nature, that eminent bed-side observer of diseases, that glory of our profession, that grand expositor, by his whole life and character, of the true nature, design, and business of a true physician.

It teaches me to take into the consideration of tissues affected by fever, the venous portion of our vascular system, left almost unnoticed by our writers, but which nevertheless plays the most important part in most of our acute and all our general chronic affections.

It teaches me to have a certain degree of reliance on the doctrine of crises, without which every physician is a rash man—and it teaches me to have respect for the experience of many clinical practitioners of great eminence and value, whose works are fallen into general neglect from the pride and self-sufficiency which distinguishes us, especially in this country, to the injury of a profession whose claims to respect depend on the labours of such men as those we despise.

It is essentially based on the law that no straight or circular fibre of the body can, physiologically, lengthen itself, but only shorten itself.

That if strong it will contract more forcibly—if weak, with less force.

That if the heart be more active it will act sooner, contract with a smaller quantity of blood its peculiar stimulus, (for I still believe Haller's doctrine of the cause of the heart's motion the best) and will consequently send a smaller column of blood into the artery, and give a smaller pulse, or smaller artery.

Gentlemen, this doctrine is the waters of Jordan—will you wish seven times, and be cleansed from your leprosy of false doctrine in fever—or will you say with Naaman, " are not Abanar and Pharfar, rivers of Damascus, better than all the waters of Israel?"

Art. VII. ***Remarks on the application of the Cold Bath in the critical stage of Country and of Bilious Fever.*** **By Thomas Y. Simons, M. D. Port Physician of Charleston, Extraordinary Member and formerly Senior President of the Royal Physical Society of Edinburgh, Member of the Medical Society, Charleston, &c. &c.**

THE success of any particular remedy in any desperate and generally fatal disease, ought to be made known, that if it be founded upon rational principles it may induce inquiry, and if it be purely accidental it may still be a dernier resort. The application of the cold bath in fever has nothing in it novel. It has been known and used as a remedy for a length of time, but it did not meet the sanction of medical men until it was so ably recommended and its effects elucidated by Jackson and Currie. A general enthusiasm (as is too often the case in medical experience) took hold of physicians, and fevers of every description and under all circumstances, were to be subdued by this all powerful regime.

The abuse from this general and indiscriminate use of this important remedy, and regarding it as the prime agent instead of a powerful adjunct in the removal of fevers, necessarily brought it into general disrepute, and it is now seldom to be heard of.

My object at present is merely to state the beneficial effects which I found from this remedy, in two cases—one, of our country fever that occurred last May, which is a highly aggravated bilious fever produced by going into the country during the hot months, and which has generally been considered fatal—and one of a highly aggravated bilious fever arising from exposure in the city, which occurred in August. In both of these cases there was in the first instance and throughout the whole disease, a great flow of bile, with all the other symptoms accompanying bilious fever. Generally in the first instance, I give a remedy which

will, according to my idea, disgorge bile from the stomach if there be any there--if not, produce nausea and diversion to the skin, thereby equalizing the general circulation and preventing an undue determination to any one particular point—and that further, after causing this change, a purgative action takes place.

For this purpose I gave, in the above cases, emetic tartar, calomel and jalap, in small doses, and afterwards a combination of cremor tartar and jalap, to carry off the irritative matter which might remain. The patients felt some relief—but I determined, as the stools were still highly bilious, to give a combination of calomel and rhubarb in small doses every hour, and a concentrated decoction of serpentaria and epsom salts, a wine glassful every hour, alternating until a change of action ensued. In both cases the disease became subdued, the bile changed, and the serpentaria always produced an agreeable moisture. But as too often happens, the patients, from the best of motives no doubt, had their stomachs overloaded with nourishment, as it is falsely termed, since nothing surely is nutritious which is not digested. This imprudent excitement of the chylopoetic viscera produced a return of fever in an aggravated form, with an increased flow of bile. Purgatives were given to relieve the obstructions and oppressions of the intestines, and the other remedies resumed, but with no benefit. Every paroxysm of fever seemed to increase in violence and force. The heat of the skin was universal, and so great in the epigastric region, thorax and head, as to make it highly unpleasant to retain the hand there, and the internal heat and thirst so great, that to use their language, they felt as if they were on fire. The arterial system was highly excited, and the pulsation of the carotid truly alarming. The patients felt a general uneasiness and restlessness, and the countenance exhibited all the perturbation and anxiety of that decisive crisis when nature or disease must quickly gain the mastery. In this state of things, any internal applications, if they could be borne, would not it appeared to me have been of sufficient power to throw off

disease, and if not, by producing great irritability in the primæ viæ, have increased it. In this contest, as it were, between the vis medicatrix naturæ and the causes which give rise to its action, there is required, it seemed, some general and powerful stimulus which might arouse (if I may thus express myself) the powers of nature, and enable her to resist and overcome those causes, whatever they may be, that oppressed the animal economy. For this purpose the cold bath seemed admirably fitted—accordingly the patients were taken naked from bed, placed in a tub seated on a bench, and five or six pails of water dashed over the head and shoulders in quick succession. The shock was prodigious. Immediately they were removed and covered with blankets, and suffered to remain in that state until a free perspiration was produced—which was continued until every symptom subsided. As soon as the general irritability ceased, the alvine evacuations were particularly attended to, until a change to a natural state took place, and light nourishments were given. The purgative medicine used was epsom salts and columbo, and epsom salts and serpentaria, and occasionally when these did not remove the sordes, cremor tartar and jalap.

The effect of the cold bath does not arise from the positive abstraction of heat, but I imagine in the following manner. The sudden cold constricts the extreme vessels, causes recession of the blood to the large arteries and heart, powerfully stimulating them to a resistance or reaction, thereby producing a general, equable and forcible action through the whole arterial system, and consequently, withdrawing its determination to any particular point, (which in bilious fever is the liver) throwing off all obstructions, and promoting a powerful and general determination to the skin. But, whatever may be the modus operandi, the phenomena exhibited in the application of the cold bath, are—first, a coldness and shivering which is short—secondly, a glow which gradually increases into great heat—and lastly, a termination in profuse sweating.

I have thus given an outline of the good results in these

two cases. I wish it however to be particularly understood, that I believe it to be only beneficial in that particular stage, and that under other circumstances its good effects are very equivocal, nay too often pernicious. I had many other cases of bilious fever which I relieved without resorting to the bath. The cold bath has been here as well as in many other places often used, but its beneficial effects were generally, I believe, considered dubious and hazardous. In this I pretend to no discovery and claim no credit. I frankly and cheerfully avow, although fully aware of its recommendations by Currie and Jackson, and Gregory of Edinburgh, I was confirmed in the trial of this remedy which I was previously disposed to use, by the advice of my highly distinguished former preceptor Dr. B. B. Simons, who had with his usual boldness and energy adopted it, in despite of the strong and unnecessary prejudices existing against the practice.

Its success in the first case induced me to try it, under similar circumstances as I have described in the second case, and with equal effect.

I cannot conclude without again stating, that I believe the cold bath to be only beneficial in the particular crisis which I have mentioned. To any other condition it is inapplicable, and probably would prove highly injurious.

CASES.

Art. VIII. *Cases illustrating the virtues of Oleum Terebinthinæ in the cure of Puerperal Fever.* Read before the Medical Society of Charleston, S. C. By Isaac A. Johnson, M. D.

CASE I.

Mrs. C. D. aged thirty-five years, was delivered of a dead child on the 19th of August 1820. She seemed tolerably well until the 21st, when she complained of severe pains in the head and abdomen, the latter being considerably tumefied and sore to the touch. She was very restless—her tongue furred, pulse tense and frequent, with a total suppression of the *lochial* discharge—nor had her bowels been evacuated since her confinement—in consequence of which, a solution of epsom salts and magnesia was administered, which had the effect of purging the bowels, but did not contribute much towards alleviating the pain and swelling of the abdomen. Visiting her the following morning, (22d) she still complained of great uneasiness about the abdomen—I therefore prescribed the spirit of turpentine and castor oil, equal parts, in doses of half an ounce every hour until the bowels were freely evacuated. On visiting her in the evening I was not a little gratified to find her greatly relieved of pains, and that she had passed a tolerably comfortable day. Thus encouraged, I continued the medicine until the following morning, (23d) when the interval between each dose was lengthened. The most alarming symptoms being subdued by the turpentine, on the morning of the 24th it was omitted, and the case treated as one of common fever until the 28th, when the abdomen becoming greatly enlarged, attended with every mark of approaching ascites, demanded my attention—the usual treatment for which was resorted to, and in due course of time she was restored to health.

Case 2.—Mrs. J. W. aged twenty-five years, on the day after her confinement, (Oct. 5th, 1820) was seized with severe pains in the head, back and abdomen, the last considerably tumefied and tender, accompanied with a total suppression of the lochial discharge—her bowels were constipated—pulse *full* and *tense*—irritability of stomach so great that the saline cathartics, though administered in small doses, were rejected. In this condition, I resorted to the oil of turpentine and castor oil, in equal parts, a half an ounce of which was given every hour until it operated freely. This had the desired effect—every dose was retained, and in a short time it operated freely, subduing the pain and swelling of the abdomen almost completely by the morning of the 7th. I considered it advisable however to continue this treatment until the 8th, when the presence of *fever* rendered it necessary to recur to some febrifuge medicine, with occasional doses of the cathartic: but little more being now required, some gentle tonic was given, and in a few days she perfectly recovered.

Case 3.—March the 5th, 1822, Mrs. J. W. was delivered of a *healthy child*. She had been much fatigued, and her mind disturbed for some days previous to her confinement, which not only rendered her labour difficult, but was the cause of a very severe illness, notwithstanding every precaution was taken to prevent it. On the seventh day after her confinement she was seized with lancinating pains about the *abdomen*, which soon became hard and sore to the touch, accompanied with severe rigors and fever, difficulty of breathing, and in short by every symptom indicating puerperal fever. This lady, having obtained relief from the turpentine on a former occasion, expressed a great desire to be allowed to take it again—but from the fulness of the pulse and fever, I preferred the saline cathartic. Apprehensive that this "new remedy" might probably be too stimulating, (being not yet perfectly satisfied of its virtues) the sub-sulphate mixture (as used in a former case) was accordingly prescribed. But on my return in the even-

ing, not finding any relief procured, and the patient still desiring the turpentine, I resolved to try it in the manner abovementioned. The medicine was taken through the night with the greatest advantage—and having slept several hours, she awoke comparatively free from pain. The medicine was continued during the next day, (14th) and on the following morning she was so much better that she sat up in bed, drank chamomile tea, and in a few days was quite restored. Mrs. W. has since repeatedly declared "that the turpentine had twice saved her life."

Case 4.—Hagan, aged about forty years, a servant of the Hon. W. J. was delivered on the 15th of May 1822, by instruments, of a *dead child*—and in consequence of the great exertions unavoidably used on the occasion, the most serious symptoms were to be apprehended. On visiting her the following morning (eight or nine hours after her delivery) I found that she could neither retain her urine nor fæces: she complained of great pain and soreness about the abdomen, accompanied by a full and hurried pulse, with pain and numbness of her lower extremities. It was suggested by Dr. J. G. that the camphorated julap with spirit of nitre and the camphorated tincture of opium, should be administered in doses of half an ounce every hour, until the most urgent symptoms were subdued. This treatment was persisted in until the evening of the 16th, when the pain and soreness of the abdomen still continuing, a large blister was applied over the whole surface of that region. On visiting her the following morning, (17th) and finding that she had obtained but little benefit from the medicine she had already taken, we believed that the turpentine might now be found useful. Accordingly a tea-spoonful of it was administered every two hours in a little milk, (the most agreeable menstruum) with alternate doses of the camphorated julap, now prepared with a larger portion of spirits of nitre. When we visited her at noon the beneficial effect of the medicine was evident, and by evening all that we could have wished for was attained. The patient had slept comfortably, which

was the first she had enjoyed since her confinement. She could now in a great measure retain her urine and fæces—her bowels had been gently evacuated—she could turn in her bed without assistance—the pain and soreness of the abdomen were much relieved, and she was in every respect much better. This treatment, with little variation, was persisted in for a few days, when every alarming symptom subsided, and the patient by means of gentle tonics became entirely well.

The following cases are somewhat analogous to the foregoing, in which this medicine proved infinitely serviceable.

Case 1.—May the 12th, 1822, I was requested to visit Nelly M'Crady, a free woman of colour about twenty years of age, then in her third month of pregnancy. Two days previous to her sending for me she had commenced flooding, and had taken several articles from a nurse without effect. She was very much reduced, and I seriously apprehended abortion. I gave her several doses of sugar of lead and Dover's powder, which were attended with no other effect than the production of constipation of the bowels. To relieve this, the spirit of turpentine and castor oil were combined and given in doses of half an ounce every hour, until the bowels were freely evacuated. As soon as this effect was produced, she slept comfortably and awoke much relieved—the hemorrhage gradually subsided, and in a few days, by the use of the volatile tincture of guaiacum, she was perfectly restored, and her *infant* was born healthy and in due time.

Case 2.—May 4th, 1823, I was requested to visit a servant belonging to Mr. John Johnson, Jr. About three weeks previously, she had received a severe blow upon the abdomen, which in a short time after produced considerable flooding and a complete cessation of motion in the fœtus, then in its fifth month. The case was alarming, although the hemorrhage had in a great measure subsided: she complained of severe pains in the head, back, and occasionally in the abdomen. She had perceived no motion in the child (except that of rolling) since the accident, and her bowels were constipated—

to relieve which, a dose of epsom salts and magnesia was administered on the night of the 4th, which though it operated tolerably well did not lessen the pains. A black and fœtid discharge issued from the vagina, which was somewhat increased on the following morning. Further evacuation of the bowels being necessary, the turpentine cathartic was prescribed in the usual proportion, directing an ounce to be taken every two hours until it operated freely. This had the desired effect—after the second dose her head and back were much relieved. The medicine was continued, though at longer intervals, until the next day, when it was omitted and a tea-spoonful of the volatile tincture of guaiacum given three times a day. Under this plan of treatment she was restored to health, and the motion of the child became vigorous.

Art. IX. *Cases of Bronchitis, with some Preliminary Remarks.* By Peachy Harrison, M. D. of Rockingham County, Va.

I KNOW not whether bronchitis in children be so prevalent in other parts of the United States as in the vicinity in which I practice. But if it be so, I have no doubt that a paper on this subject would be very acceptable to many members of the profession, and more especially as no one I believe has given a full and satisfactory account of the symptoms and treatment of this very frequent and formidable disease. I have indeed sometimes wondered why some of the masters of our art had not supplied this chasm in medical literature, since so much has been written on croup, a disease within the circle of my acquaintance less common, and not at all more intractable than bronchitis. In the earlier part of my practice, I must be candid enough to

confess I could not distinguish the worst forms of bronchitis from croup, and this may be one reason why the latter disease seemed to be so prevalent at that time. But it is very remarkable, that for the last twelve years I have not seen more than five or six cases of tracheitis, while the disease under consideration has occurred with great frequency.

In this paper I have nothing else in view than to give the result of my own observations and experience in this disease.

Bronchitis occurs at every period, from the earliest infancy to the third or fourth year of childhood: yet from what I have seen, much more frequently in the first and second year than subsequently—though some of the worst cases I have met with have been in children of the third and fourth year.

This disease is commonly occasioned by sudden changes of temperature, producing in the delicate mucous tissues of the bronchia, probably in the first place torpor, succeeded in a short time by a state of irritation. This state continues variously from a few hours to two or three days, and then passes into inflammation—which if it be of the acute kind, lasts only for a day or two at most, and is succeeded by collapse and depression, which usually eventuates sooner or later in death. The chronic or sub-acute form of the disease does not so clearly reveal these stages—those of irritation and inflammation are not so distinctly marked, and that of collapse does not supervene.

These views of its etiology and pathology being just, bronchitis naturally divides itself into the three stages of irritation, inflammation and collapse, each of which having its peculiar symptoms and treatment.

The stage of irritation is marked by the usual symptoms of catarrh. As a natural consequence, there is an increased afflux of fluids to the irritated tissues and an increased secretion of mucus, giving rise to a frequent hurried cough and some degree of dyspnœa. During this stage, infants at

the breast suck very well, and children weaned eat and run about as ordinarily. The diseased manifestations indeed so much resemble those of common cold, that they excite little uneasiness in parents. But an experienced observer may anticipate the accession of the stage of inflammation, by the greater frequency of the cough, the greater abundance of the mucus, and the greater confinement of the breathing than in common catarrh.

The stage of inflammation is announced by a reaction of the system, by a great increase of dyspnœa, by a frequent dry cough, by a prostration of muscular strength as evidenced by a disposition of the patient to a recumbent posture, by loss of appetite, and by a tongue covered with a white fur—to which we may add the unequivocal indications of pain in one of the sides, commonly the right. The respiration in the worst cases is very much restricted, and accompanied with a hoarse sound, which however the skilful practitioner may very readily distinguish from the croupy sound of tracheitis.

The conversion of irritation into inflammation, which is to be regarded as only an exalted degree of the former, and may therefore happen by a very natural and easy process, gives rise to this new train of phenomena. The heart and arteries are roused into sympathetic actions by the phlogosis of the mucous membranes of the bronchia. Much observation has conducted me to the conviction, that in this disease, if not in every other where the heart participates in the diseased actions, fever is symptomatic of a local affection. I am not fully satisfied there is no such thing as an idiopathic fever, though I have long since been convinced that in bronchitis, in croup, in cynanche maligna, and in several other diseases, the constitutional is the consequence of the local irritation. But it would be quite foreign to my purpose, at present, to pursue further this pathological discussion. The alarming dyspnœa, the dry cough, the stitches in the sides, are natural consequences of a phlogosed state of the bronchial vessels, rendering them undilatable and

occasioning a dissipation of the thinner parts of the mucus, if not lessening its secretion.

The third stage or that of collapse succeeds to the second in a few hours, or in a day or two at most. It is distinguishable by a total disappearance of febrile heat, followed by a coolness and some degree of lividness of the extremities, and very often of the face and whole surface, while the pulse is feeble and very frequent, and the breathing still more laborious than in the second stage--the cough being sometimes wholly suppressed, and when it is not, is very much confined and even suffocating.

The pathology of this stage, it seems to me, is to be sought n an extreme concentration of debility in the bronchial tisues, leaving the rest of the system, and especially the tomach and liver and other chylopoetic viscera, in a state f inaction, as is fully attested by the great unsusceptibility f those parts to the impressions of medicines, as will apear in the sequel.

It ought to be remarked, before we close the history of ronchitis, that as in the chronic form of this disease, these tages are not as distinguishable, so the attacks are someimes so sudden and violent that the discrimination between he first and second is not very easy, and seem to be merged n the third. In these cases the skin never becomes hot, ut on the contrary is cool from the first, the breathing exremely oppressed, and the cough suppressed, and without peedy relief death soon follows.

In children, bronchitis can only be confounded with traheitis, and it is very likely that a practitioner not conversant vith either of the diseases, might readily mistake the one for he other. In each a very difficult respiration is the most rominent symptom, it being hard to say in which it is the nost so, and in both death is the consequence of suffocation. But in bronchitis, the peculiar cough and breathing which o impressively characterize tracheitis is not present—the ough, though hoarse and exceedingly confined, has not the vhizzing, barking sound so characteristic in croup, and

which once heard can never be forgotten. In the latter disease the stricture in the respiratory apparatus is clearly referrible to the upper part of the windpipe, while in the former as certainly to the lungs. We are informed by high authority that croup may change into bronchitis. This is possible, though no instance has ever occurred to me in which it unequivocally happened—yet taking place as on the authority referred to, I have no doubt it has: it will then be difficult if not impracticable to distinguish these diseases. The diagnosis, however, becomes unimportant, since the treatment will be the same. What also is very generally attendant on bronchitis and is absent in croup, is a soreness and perhaps pain in the right hypochondrium. This indeed so generally exists, that German women in this country give this disease the appellation of *liver-grown*, by which they mean to express an enlargement of the liver. Whether this enlargement really exists I am not prepared to aver or deny. I have however a strong suspicion that the liver has some participation in this disease, and which has arisen not at all from the vulgar notion on the subject. It is a fact, that in almost every instance there is great soreness in the right side, and that a copious evacuation of green bile very often announces a favourable solution of the case.

Next, I am to make some remarks upon the therapeutical measures, which I have found most successful in this disease, as applicable to its several stages.

In the commencement, bronchitis might generally be hindered from passing into the second stage by a little attention on the part of nurses. The patient being kept within doors, with a pediluvium at night—or if the breathing should be more difficult than is usual in a common cold, and phlegm abound, a gentle emetic would arrest its progress. But the physician very seldom sees the case until the second stage has come on, and then he will find it necessary to employ the most prompt and decisive measures. Now he must bleed largely so as to abate the fever and dyspnœa. But often venesection cannot be accomplished to any useful extent, owing to

the intrinsic difficulty of the operation in infancy, and for other reasons. In either case whether blood has been let or not, it is my custom to give a combination of tartrite of antimony and sub-muriate of mercury in reiterated portions, until both vomiting and purging are induced. In this and in the third stage of bronchitis, I place very little reliance on emetics alone, pretty ample experience having taught me that they afford only slight and temporary relief, and which is more especially true in the worst cases. Yet I would almost always use them in connexion with purgatives—not so much with a view to their emetic effects as to the activity which they give to purgatives. My great object in such cases is to make a strong and permanent impression on the alimentary canal, including the rest of the chylopoetic viscera, and to accomplish this purpose, I know of no articles in the materia medica so much to be relied on as those mentioned. The precise manner in which I prescribe them will be discovered in the account of the cases subjoined. These failing however to operate forcibly on the bowels, jalap will prove the best auxiliary, except in early infancy, where castor oil is preferable.

These remedial agents used with proper boldness and perseverance, by impressing forcibly a large surface of mucous tissues in the stomach and bowels, cause a powerful revulsion from that of the bronchia. Generally when large discharges are procured from the bowels, all the symptoms are at once meliorated—the febrile heat subsides—the breathing becomes more full and free—and in short, the little sufferer seems snatched from a premature grave.

Applied to the breast and sides, rubefacients are useful. Yet nothing is of so much importance to secure complete and final success, as a continued exhibition of calomel in doses from one to three grains every hour or two, with an occasional dose of castor oil to procure a full operation on the bowels.

Not seeing the patient until the third stage of the disease, the practitioner has to contend with the most formidable difficulties. The vital movements seem to recede from

the surface, and are determined to the pulmonary apparatus: indeed there is apparently here, a concentration of nearly all the vital phenomena, as is evinced by the very feeble action of the heart, and by the almost insuperable torpor of the chylopoetic viscera, requiring the largest quantities of the most active medicines to arouse them to activity. This part of the subject will be illustrated by a few cases which I mean to adduce presently.

In this case of bronchitis no one would think of bleeding, and blistering is too slow a process. The warm bath has appeared to me to increase the exhaustion without having the least power to restore proper action to the surface, or equalise vital susceptibility and excitement through the system. As far as my experience goes, there is only one measure upon which we can rely with any degree of confidence. It is to abstract action from the mucous membranes of the bronchia, and transfer it to the stomach and bowels, and through them to the liver and other abdominal viscera. This must be done by a bold and persevering administration of the sub-muriate of mercury with tartrite of antimony, jalap, castor oil, &c. and followed if necessary with stimulating enemata.

It is truly astonishing and delightful to observe what a calm succeeds to the most painful and distressing agitations, after a full and free evacuation of the bowels. This it is true is not always the case, and then it is ominous of a fatal issue. It is under such circumstances peculiarly necessary to make reiterated and strong impressions on the alimentary canal, and by means of calomel chiefly. I do not recollect that I have ever used the tartar emetic successfully, except in attempting to excite, at first, the stomach and bowels.

The dyspnœa not being removed by such means, I have always and beneficially had recourse to vesicatories. My experience may be fallacious, but in my practice I have seldom had occasion, in the acute forms of bronchitis, to resort to them. The remedial measures chiefly insisted on, having their full effect, they will seldom be necessary: if not,

their assistance, admitting them to have the requisite powers, will come too late. The fact is, that in the stage of collapse, excitability is extinct on the surface, and vesicatories seldom produce any effect. Besides, were they supposed to act well, it is very questionable whether the small impression induced by them on the skin, would much relieve the internal oppressions: but often an equilization of excitement has been commenced, by a strong irritation applied to the mucous membrane of the whole tract of the alimentary canal, and through this to all the chylopoetic viscera, and then epispastics may prove useful auxiliaries.

In the milder forms of the disease, the same remedies used in much lighter doses will be successful. Commonly I have found it sufficient to give a dose or two, composed of a grain of emetic tartar, and from four to six grains of calomel, to be repeated until vomiting and purging are induced.

In the chronic or sub-acute form of bronchitis I have used venesection, repeated emetics, purging with grain doses of calomel, epispastics, the squills, and digitalis.

I propose now to illustrate the nature and treatment of this alarming disease by a few cases which have occurred to me within the last four years: these I select out of very many others that might have been cited, as among the most violent, and because more full memoranda of them happened to be preserved.

CASES.

On the 28th of April 1818, I was desired to see a child of Thomas Maguire, aged between three and four years, of a full habit and previously very healthy. He had been for a few days labouring under symptoms of common cold, without occasioning any other than a slight indisposition. On the day before I saw him he had fallen into a spring of water and became very wet, and on the following night a violent fever supervened, accompanied by great obstruction of the respiratory apparatus and suppressed cough. I saw him the following evening in a state of entire collapse—the whole

surface cold, pale, and rather livid, with a clammy sweat on his face—his pulse scarcely perceptible, and breathing extremely laborious and somewhat hoarse, attended by a very slight cough. Thinking the case entirely hopeless, I was still, at the request of parents and friends, induced to make an effort to save the child. I ordered a warm bath, and began to give a combination of four grains of tartar emetic with about a scruple of calomel in divided parts. He could sustain the warm bath only a very short time, and was greatly exhausted after being removed from it. The first parcel of the powders was given in the course of an hour, though without any obvious effect—another of the same kind was prepared and exhibited in the same manner in the course of the next hour. During this term, prospects seemed rather more discouraging, when suddenly and unexpectedly the medicine produced a very powerful impression on his bowels, bringing away a very copious stool, soon followed by others. Not long after the first passage the breathing became much easier, and the general agitations of the system nearly subsided: warmth and some degree of floridness returned to the surface, and in short safety succeeded to the utmost danger. Light doses of calomel with syrup of the polygala senega, repeated for two or three days, completed the cure.

Case 2.—On the 12th of December 1821, I visited the daughter of Mr. Robinson, aged about ten months, still at the breast, and received the following history of the case:—that ten days ago she had been taken with symptoms of a violent cold, which had not suffered any material aggravation until a day or two before I saw her. At this time there was an accession of fever, accompanied with great dyspnœa, violent cough, &c. The stage of fever and inflammation had passed away, and that of collapse succeeded. The respiration was exceedingly difficult, threatening immediate suffocation. The extremities were cold and livid—the pulse frequent and small—had been unable to draw the breast for several hours, and deglutition was much impeded.

Taught by the successful issue of former cases which had

been deemed very desperate, I determined to pursue the same vigorous practice. Being fully aware of the extreme torpor of the stomach and bowels in such cases, I prepared a mixture of four grains of emetic tartar and eight grains of calomel with a proper quantity of syrup, of which a small portion was given every ten minutes until the whole was taken. Not producing the least visible effect, a similar mixture was prepared and given more freely than the former. It now became obvious that the medicines were making impressions on the alimentary canal, though neither vomiting nor purging had taken place at the end of two hours. To promote purging, on which I supposed every thing depended, I gave at once forty grains of jalap, and had the satisfaction to see the operation of it in half an hour. The evacuations were very bilious though not copious, and were attended with a discernible, though not very marked improvement of the symptoms. Having blistered the breast, small doses of caomel were daily repeated for two or three days, and the child gradually recovered.

Case 3.—On the 16th of August 1822, I called to visit a daughter of Mr. John Florey, aged about three years, and was informed that she had been labouring under symptoms of catarrh for two weeks before. The attack, which was about noon of the 18th, disclosed itself by a violent vomiting, soon followed with great dyspnœa and pyrexia. I found her labouring under deeply oppressed breathing, the skin cold and rather livid, the tongue as if parboiled, the pulse feeble, a short confined cough, the epigastrium distended, the bowels torpid, having had no passage for two days, and great jactitation.

Her parents were under an impression that these symptoms were owing, at least in part, to her having eaten largely of water melons the day before, which had been kept in a spring house in very cold water. This conjecture was not without probability.

The time for blood-letting had obviously past. By previous experience I had been taught that this operation, under such circumstances, rather augments than arrests the disease,

by still more exhausting the already too much emptied aortic, without at all relieving the congestion of the pulmonary system of vessels.

I therefore determined at once, as in former instances, to make if possible an active impression on the stomach and bowels: for this purpose, fifteen grains of calomel and two of tartar emetic were mixed in some sugar and water, and a tea-spoonful given every ten minutes. Before the medicine was half taken vomiting was excited, a circumstance not anticipated, as I had uniformly found it very difficult and sometimes even impossible, to induce it. This was followed by no operation on the bowels, and without any relief of the dyspnœa. As soon as the vomiting ceased fifteen grains of calomel and twenty of jalap, mixed with some syrup, were given in portions at short intervals. No purging taking place, another portion of the same medicine was prepared and given twice in the hour. None of it was rejected by the stomach, and the intestinal canal now began obviously to be impressed, and as an effect, after one or two small evacuations, the breathing was perceptibly easier. I now left my patient, with directions to give occasionally three or four grains of calomel, unless rendered unnecessary by the active operation of the medicine already taken—and in the event of the dyspnœa not being quite relieved, a blister was to be put on the breast. The child however grew better, as the bowels were more copiously evacuated, and finally recovered. In this case the superiority of purgatives over vomits, in relieving the symptoms of bronchitis, and especially in the stage of collapse, is clearly manifested.

Case 4.—On the 9th of December 1822, Mr. J. B—— called on me to prescribe for his daughter, aged about twelve months, who he informed me had been taken ill three or four days previously with shortness of breath, fever, &c.—that the symptoms considerably abated after the first day until the evening before his call, when all returned with great violence. Conceiving the case to be bronchitis, I prescribed my usual emetico-cathartic portion, which I was in

formed operating well, gave great relief, and which continued till late in the evening of the 10th, when the dyspnœa and hoarseness began to return. On the 11th I was desired to see her, and found the respiratory apparatus greatly oppressed, and breathing attended with a whizzing noise in the trachea, though not so distinct and remarkable as in cynanche trachealis. The skin was cool and somewhat livid, especially on the extremities—the child was unable to suck—had great aversion to swallowing every kind of liquids, and some difficulty in doing so—all which was attended with much restlessness.

I was unable to determine from the history of the case, as collected from the parents and others, whether at first it was bronchitis or croup. But I thought it most probably the former, combined now, however, with an affection of the larynx. The stage of excitement had been over for some time, and so desperate did I consider the case, that I hesitated some time whether I should make any attempts to save the child. But recollecting the alarming condition from which I had seen patients of this kind snatched on former occasions, I concluded to make an effort, and began with the warm bath, the water being as hot as it could be borne, with continual frictions on the skin while in the water. The child came out pale and exhausted, though kept in only a few minutes, and with very slight relief. I hoped however it would serve to recall some degree of excitement to the surface, and perhaps render the system more susceptible to the operation of medicines. As soon as the child was taken out of the bath, small but often repeated doses of tartar emetic and calomel were given. Many ineffectual efforts to vomit were made in the course of an hour, with some alleviation of the breathing. This however was only transitory. The medicine could no longer be exhibited, and she died on the 12th.

In this case I think it is very likely, that if the impressions made by the first medicines had been followed up by other measures, as blisters, the polygala senega, repeated small doses of calomel, the disease would have been subdued.

Of venesection in bronchitis I have said very little, because in the worst forms I have seldom had an opportunity of trying it, and with milder forms it is seldom required.

In the chronic form of the disease, I have used blood-letting, repeated light emetics and blistering, which two last remedies have succeeded best in my hands. The tincture of digitalis has seemed to me to be useful, and half grain doses of calomel given daily will be found of singular benefit.

To attacks of bronchitis, some children are much more disposed than others. Such ought to be made to wear flannel next the skin, except in the heat of summer, and to be carefully, in every respect, guarded against the vicissitudes of weather. The symptoms threatening an attack should be immediately met and removed by a pediluvium, by a few tea-spoonfuls of sweet oil, or a little garlic juice, or if necessary, by a gentle emetic or a few grains of calomel.

We early adopted and have regularly continued the practice, of recording the most interesting cases of diseases which come under our notice. As some of the cases in our collection may be of value, we propose from time to time, to introduce them into the pages of this Journal. The one now presented, we think, in several views, has claims to attention.—Editor.

Art. X. *Case of Inflammation of the Vessel from Venesection, which terminated fatally.* By N. Chapman, M. D.

In the month of March 1810, I was requested by the late Mr. Thomas W. Francis, of this city, to visit his coachman. On inquiring into the case, I learned that three days before, while on a journey, he had been attacked in the

evening with slight symptoms of pleurisy, which very readily yielded to a moderate bleeding.

The next morning he felt for the first time, some degree of pain and tension in the right arm, in which he had been bled. But the uneasiness was so trifling, and in other respects he was so well, that he continued on the journey. The exertion of driving, as might have been expected, aggravated exceedingly these affections. Yet such was his anxiety to reach home, that he studiously disguised his real situation, lest he might be left behind—and obstinately persevered against every remonstrance, to perform his duties as coachman. But on the last day of the journey, overcome by the severity of his sufferings, he reluctantly consented to be placed in the carriage, and was in this way conveyed to the city.

My attendance on him commenced in the night, an hour or two after his arrival. Even at this early stage, the case presented a very serious aspect. The arm was swelled to perhaps twice its natural size, and the pain and inflammation were excessive. By pressure, a copious stream of purulent matter issued from the orifice, and I could distinctly trace the enlargement of the vein for several inches, imparting the sensation of a hard inelastic tube enclosed under the integuments.

Nor were these the only untoward circumstances of the case. There was also considerable pain in the left side, and a universal soreness pervaded his body. Little or no fever was indicated by the pulse, which was weak, irregular and quick, of a contracted volume, and rather corded. It was more a *disturbed* than a *febrile* pulse—certainly evincing nothing of an inflammatory diathesis. But though apparently not much, if at all feverish, he was greatly harrassed by restlessness and inquietude. The temperature of his body was unequal and fluctuating. When his attention was fixed by conversation addressed directly to him, his mind seemed perfectly rational: but otherwise, he quickly became flighty, talked incoherently, and endeavoured to get

out of bed. Whatever were my doubts as to the ultimate event, I entertained none respecting the nature of the complaint, or the causes which had produced it. I was at once satisfied, that the whole of the existing mischief was attributable to inflammation of the vein, extending probably to the heart, and to the introduction of pus into the blood. To the latter cause I the more promptly imputed the train of nervous affections, as I had seen in a series of experiments, phenomena of the same kind induced by the injection of pus, of milk, of oil, of mucilage and other bland fluids, into the veins of different animals.

Nor could I hesitate long as to the practice to be pursued. To subdue the inflammation of the vein, and arrest the pus in its passage to the circulation, were obvious indications.

Notwithstanding the feebleness of arterial action, I bled him to the amount of twelve ounces. I suspected a state of depression, and thought it not unlikely that the pulse might rise by depletion. Where so large a vessel was inflamed, and seemingly too, the heart itself, it certainly was not unreasonable to conjecture that the want of diffused excitement was owing to this condition of the system.* But my anticipations were not realized. The bleeding was indeed followed by no sensible effect, and the blood was without any very peculiar appearances. It did not it is true, separate as it ordinarily does. The serum and crassamentum were commixed, as if slightly stirred together. I could detect no pus in the blood.

I next enveloped completely, the arm with a blister from the elbow to the shoulder,† excepting at the puncture, which was covered with a small emollient poultice to facilitate the

* We are indebted to Sydenham for the important fact, that in many instances there is a *depressed*, in contradistinction to an *exhausted* system, and which is to be aroused not by *stimulation* but by *depletion*. Brown, who had his *direct* and *indirect* debility, differs from him in maintaining that each of these states is to be relieved by stimulants properly graduated.

† There is nothing I believe in the case of an inflamed vein so efficacious as blisters. They often operate promptly and decisively. We are indebted for the practice to Dr. Physick.

evacuation of the matter. During the night, I ordered, moreover, that he should take at stated intervals a solution of salts till it purged him actively.

In the morning he appeared in some respects to be better. The blister had drawn well, and the pain and swelling were in consequence considerably reduced. Neither was he so restless nor irritable, and his mind had ceased to wander.

When at night I again called, he was much as he had been at my preceding visit. The swelling of the arm was perhaps somewhat further abated. But in a few hours afterwards, the pain in the side, which at no period had entirely subsided, reverted with violence. It did not however *raise his pulse!*

Convinced by a variety of considerations, that the pain proceeded from inflammation of the heart, I placed a large blister over the region of that organ. Though relieved by this application, of the pain, I had the mortification of seeing him the succeeding morning, in a situation of increased danger. With a pulse weak, quick and tremulous, he was wild and distracted. I directed that he should lose six or eight ounces of blood by cups, from the neck and temples, and to have a blister put on behind each ear.

It now appeared to me to be literally of vital importance, to intercept the pus in its course to the circulation. I therefore resolved without delay, to make use of compression, though it seemed still to be forbidden by the tumefaction of the limb. A bandage and compress were accordingly applied, a short distance above the puncture. But nothing material was gained by these applications. The swelling of the arm however declined, and no inconvenience was experienced from the compression. The pus continued to flow profusely from the orifice: this was late in the evening.

My visit the next morning found him worse. To all the bad symptoms which previously prevailed, were now added, some still more inauspicious. The morbid sensibility of his body had become so exquisite, that he could not bear the slightest touch, or scarcely the weight of the bed clothes,

without complaining. So sensibly alive was he to every sudden impression, that by opening or shutting the door, or walking across the room, or by a question put to him in a sharp tone of voice, or by a strong glare of light, he was startled, and sometimes exceedingly agitated.

At this critical juncture I resorted to the advice of Dr. James, who very obligingly met me in consultation. We agreed to give the camphorated emulsion in large doses, and to have stimulating injections repeatedly administered. But this treatment was equally unavailing. My patient progressively sunk. Low delirium, cold extremities, tremors of the nerves, and convulsive cough, soon supervened. His pulse became hardly perceptible. The pupils of the eyes were widely dilated, and his countenance assumed an expression uncommonly haggard, phrensied and distressed. Desperate as I deemed the case, I did not permit my exertions to be relaxed. By the constant use of the most powerful stimulants, such as camphor and opium, the volatile alkali, the spirits of turpentine, ether, wine and brandy, I protracted his existence for three days longer, without having, however, during this interval, a single gleam of hope afforded me, by any change, of his recovery.

The morning after my patient expired, I made an examination, with a view of ascertaining the exact state of things. I exposed the vein from the wrist to the axilla. The external surface of the vessel was in many places inflamed, and especially above the puncture. Betwixt this and the shoulder, matter had penetrated through the vein by four distinct sinuses into the neighbouring cellular membrane, forming small abscesses. Two of these sinuses were high up the arm.

I dissected very carefully the parts adjacent to the wound. Directly around it there was an abscess containing, I presume, a large spoonful of pus, mixed with a dark fetid sanies. Sphacelus had already destroyed a portion of the cellular texture.

I next laid open the vein. There was inflammation more or less, from a little below the elbow to the final point of my

dissection—without being any where very great. By the appearance of the coat of the vein there was, however, the amplest proof of its having existed in the highest grade, and which is abundantly shewn by the formation of the sinuses, &c. Below and above the orifice for several inches, gangrene had taken place, bounded by an extensive erysipelatous blush, and the inner surface of the vessel within this space, had begun to slough.

The quantity of pus in the cavity of the vein was small. It ought, indeed, to have been stated, that for two or three days prior to his death, the discharge from the orifice had gradually diminished. No disposition whatever was evinced any where along the canal of the vein, to an adhesion of its sides. My wish to extend the dissection was frustrated. Enough, however, has been brought to light by this partial exposition, to confirm my original notions as to the nature and causes of this series of extraordinary affections.

In reflecting on the management of the case, I have only to regret having confided too much in compression as a means of promoting adhesion. But surely I shall escape censure, for having adopted a measure which had been approved by the very high and concurrent authority of Hunter, Cooper, Abernethy, &c. &c. I had never before an opportunity of trying compression under such circumstances, or of seeing it employed. My conviction now is that it will rarely succeed. It is confessedly at all times difficult to produce a union in tubular or fistulous ulcers, even where pressure can be used. It is yet more so, to effect a coalescence between the opposite surfaces of blood-vessels, because among other obstacles, the coagulable lymph which is the medium of attachment, must in a great measure be swept away by the circulation as fast as it is deposited. It is besides almost impracticable to make adequate compression on any one of the veins of the arm, without interrupting the return of the blood. I confess however that these speculative objections to the practice, have less weight with me than the melancholy instance of its failure which I witnessed. In the case of my patient the experiment was fairly made—for five days

and nights successively I continued the compression, and with the utmost vigilance to the due regulation of it. Distrusting, therefore, the efficacy of this expedient, if ever I should meet with a similar case I would apply *a ligature to the vein.* The operation however will not often be necessary. It can only be required where suppuration has taken place, and there are grounds for the apprehension that the matter is travelling into circulation. Cases of this sort are extremely rare. Few, at least, have been recorded. I have met in my researches with only one at all analogous.* They have hitherto been contemplated by writers rather as a *possible* than an *actual* occurrence.†

Injuries to the veins from bleeding, are for the most part attended by only circumscribed inflammation. The suppurative process seldom ensues. Even when it happens, it is generally limited to the vicinity of the wound, through which the pus is either freely evacuated or is lodged in the surrounding cellular texture, and the abscess thus formed may be made by an opening to discharge externally, if contrary to the ordinary course, it does not do it spontaneously.

* This case is related by Mr. John Hunter. In dissecting the arm of a man, he traced a series of adhesions along the course of the vein. But near the axilla the vein, says he, "had taken on suppuration, beyond which adhesions had not formed, and this had given a free passage for the matter *into the circulation, of which the patient most probably died.*" It is to be regretted that a case apparently so interesting, should be so imperfectly detailed.

† Mr Abernethy, in his essay on this subject, tells us that he had never seen a case of the vein suppurating from venesection. But he *can conceive one,* in which it might even be proper to divide the vessel to prevent the pus from entering the circulation. With the greatest deference to the authority of this distinguished surgeon, I cannot help giving a preference to the ligature in those cases. It has all the advantages of the operation which he proposes, and is exempt from the hemorrhage and other inconveniences which might attend it. Nor does Mr. Charles Bell, another surgeon of great eminence and experience, appear to have met with such a case Treating, in his system of operative surgery, of suppurating veins, he says, "the danger here is *conceived* to arise from the matter formed within the veins being carried into the circulation," &c. In fine, all the writers whom I have consulted, hold the same sort of language upon this subject.

REVIEWS.

Art. XI. *Essays on various Subjects connected with Midwifery.* By Wm. P. Dewees, M. D. 8vo. pp. 479. Philadelphia, H. C. Carey and I. Lea.

WE confidently hope that no one, at all acquainted with the present condition of medicine in this country, will be ready to accuse us of presumption, when we state it as our firm belief, that the science and practice of Midwifery have attained as high a degree of perfection here as on the other side of the Atlantic. Nor do we fear of being charged with giving unmerited praise, when we advance, that among the men who have contributed most to raise them to that state, the author of the present volume stands pre-eminent. To point out to the physicians of our country, the merits to which Dr. Dewees is so justly entitled as an enlightend, ingenious and sound accoucheur—as the author of many valuable improvements in the practice of the art, and as a learned and accomplished physician, would be a work of supererogation, since his reputation is already sufficiently established among them, by means both of the excellent lectures he annually delivers on the subject of midwifery in this city, and of his writings. But to the medical world abroad, where access cannot so easily be obtained to American publications, and especially those of an early date, some account may be due of his valuable labours. With this intention, we proceed to offer an analysis of the volume, the title of which we have given above. It is entirely made up of essays originally published in the various periodical works of this city. These essays are chiefly upon practical subjects, and it cannot be a little to their recommendation to state, that they are for the most part, the results of an extensive practice in midwifery for four and thirty years.

We proceed to our analysis.

The first essay which offers itself, and to which we shall call the attention of our readers, is on *superfœtation.* Dr. Dewees is a decided advocate for the possibility of this phenomenon, and in support of his opinion has brought forward not a few ingenious arguments—and what will appear much more satisfactory to all, has presented us with two cases which must be regarded as highly interesting, and as going far to prove the correctness of his position. In the first of these cases, the woman was delivered of a fine healthy boy after a labour of some hours, and a short time after, of an ovum containing an embryo of between three and four months.

"The following considerations," says Dr. Dewees, "will establish beyond doubt that it was a case of superfœtation:

"1. The absence of hemorrhagy during the whole period of gestation, which would not have been the case, had the placenta been any time detached before the period of labour.

"2. The ovum having nothing in common with the full grown fœtus; on the contrary, it had its own membranes, water, placenta, &c.

"3. The fresh and sound appearance of the ovum.

"4. It having maintained its attachment to the uterus, after the birth of the other child, or at least it did not descend so as to be discoverable by a careful examination of the vagina and otherwise, which renders its attachment more than probable, since this must and would have happened by the common tonic contraction of the uterus after the birth of the other child and placenta; and that the uterus did contract is certain, as no hemorrhage followed the extraction of the placenta."

The second case which is related, is that of a white woman of Abington near Philadelphia, who was delivered of twins, one of whom was perfectly white, the other perfectly black. The latter of these had "all the characteristic marks of the African—short stature, flat, broad nosed, thick lipped, woolly headed, flat footed and projecting heels," whilst the former was delicate, fair skinned, light haired and blue eyed.

Dr. Dewees rests his principal arguments, in favour of the possibility of superfœtation, on the peculiar views he enter-

tains respecting the passage of the semen from the vagina to the ovaria. Rejecting, and we may confidently say refuting, the old theories of the direct passage of the prolific fluid through the cavity of the uterus and the fallopian tubes, he advances the supposition that this fluid may be conveyed to the ovaries by means of absorption.

"This absorption," he says, "may be effected in one of two ways; first, either by the common absorption of the vagina taking up the semen and going the route of the circulation; or secondly, by a particular set of vessels which we shall call seminal absorbents, and which have a direct communication with the ovaria. With respect to the first it cannot well happen in this way, as every thing taken up by the absorbents is subjected to their alterative influence, and in this case the peculiarity of semen would be immediately lost by the exertion of this power; nor is this difficulty obviated by supposing this change necessary to the end, since it has been proved by Spallanzani that the absolute presence of the semen in its unaltered form is essential to impregnation. Therefore we are inclined to believe it to be in the latter way; as it would seem to agree better with the general simplicity of nature."

At the time Dr. Dewees advanced the preceding opinion, the existence of this peculiar set of seminal absorbents was merely conjectural—no one having as yet demonstrated, or as far as was known to him, intimated a belief of them. Within a very few years, however, the doctrine founded on them has received no small degree of confirmation by the labours of Dr. Gärtner of Copenhagen, who discovered in some animals a duct leading from the ovary to the vagina.

"It may be asked," says Dr. Dewees, "if there be this particular set of vessels within the vagina for the express purpose of taking up the semen, why do they not also absorb the matter of gonorrhœa or lues, and thus produce the destruction of the ovaria by conveying it to them? To this we might answer that such may be their economy or dispostion, that they are only roused to absorption by their own particular stimulus, namely, the male semen."

The succeeding essay is entitled "*An examination of Dr. Osburn's opinion of the physical necessity of pain and difficulty in human parturition.*"

"Dr. Osburn, in the introduction of his essay on laborious parturition," says our author, "has endeavoured to prove that pain and difficulty are natural to women in parturition. He conceives 'that in sorrow shalt thou bring forth children,' was a curse pronounced by God against man, and that it was his intention it should be fulfilled and continued as long as the world endured. That this curse was felt and perpetuated by the erect form which he gave man; while the horizontal one of the subordinate quadruped exempted it from these evils; he also supposes 'that the peculiar advantages of positions so different from each other, can no more exist in the same creature, than the strength of the draft horse and the fleetness of the racer can be united in the same animal; as these depend on qualities incompatible with each other, and which cannot exist together in the same subject, so those depend on circumstances of structure or physical laws equally incompatible and inconsistent.' "

From all these positions Dr. Dewees differs. He remarks first, that the physical necessity of pain and difficulty is by no means proved by the text which Dr. Osburn has brought forward to demonstrate it. He contends that "in sorrow shalt thou bring forth children," does not necessarily imply that they shall be brought forth with pain and difficulty—for sorrow is in no one instance in the holy writings made synonymous with pain and difficulty, or made to signify corporeal sensation—but that on the contrary, it is invariably used to express a certain painful state of the mind. He consequently thinks it was intended to express that anxiety which every woman feels for her own safety, and for that of her infant, at the interesting moment of becoming a mother.

"This I conceive" says our author, "is the true meaning of the text quoted by Dr. O.—for were it otherwise and made to signify pain and difficulty, it would necessarily imply punishment—this punishment ought universally to obtain, agreeably to the intention with which it is said to have been inflicted; but this is not the case; we therefore cannot suppose it was intended as a punishment. On the contrary, it is more than probable that pain and difficulty are artificial, and are the consequences merely of civilization and refinement. For the human constitution, when not under the influence of these causes, will, cæteris paribus, be found capable of meeting and overcoming without any difficulty, the ordinary changes produced by gestation and delivery.

"The mischief derived from the sources just mentioned, (civilization and refinement) are found to consist in the disposition to, or existence of diseases, either general or local, or both; in those which may affect the system in general, or those which may be confined to the uterus or pelvis in particular; in the introduction and continuance of certain pernicious customs, habits or modes of life, thereby inducing a preternatural degree of irritability, sensibility, laxity or rigidity—and hence the physical necessity of pain and difficulty in parturition, among the greater part of women in a state of civilization and refinement. The difference then in the opinions of Dr. O. and myself, consists in what he supposes natural and unavoidable, I believe artificial and in part remediable."

We hope it will not be viewed in any other light than as an act of justice to notice, whilst on this subject, the injustice which has been done to Dr. Dewees by some of the European writers, though more particularly by Mr. John Power, who in a treatise on Midwifery, (the first edition of which was published in London in 1819,) has defended the same opinion which we have just seen was advanced and supported by our author—namely, that in the natural state of society, pain and difficulty are not natural to women in parturition—and who in doing this, has employed nearly the same arguments as those in the essay under review, more fully developed in his Essay on the means of lessening pain, first published so early as the year 1806. It is indeed impossible to examine this latter work and compare it attentively with that of Mr. Power, without being at once struck with the sameness of the principles defended in them, and at the same time feeling convinced of the source from which he derived his views. Mr. Power not having thought fit to give the credit of originality where it was due, or even to make mention of Dr. Dewees' name, we have regarded it our duty to make the claim, and at the same time to protest against the assumption.

Dr. Dewees next proceeds to examine separately Dr. Osburn's arguments in favour of this supposed natural physical necessity of pain and difficulty. Into this examination however we cannot follow him, both from want of sufficient room, and from a desire that his observations should be read in the

work itself. It will suffice for us to remark in this place, that in our humble opinion, he has completely refuted his adversary in all his positions, and succeeded in establishing the correctness of his own opinion—that pain and difficulty are not physically necessary, but the artificial product of civilization and refinement.

The next essay contained in this volume is entitled "*Observations on Dr. Denman's Aphorisms on the use of the Forceps*," which in every respect merits to be studied by the student and even practitioner of midwifery. In this essay Dr. Dewees has, we believe, satisfactorily shewn the many errors and false precepts contained in the Aphorisms, and in doing this has conferred no small obligation on the younger members of the profession, who in but too many instances continue to follow scrupulously the rules laid down by Dr. Denman for the use of the forceps. We should be very sorry to be understood as undervaluing the reputation, which Dr. Denman has so deservedly acquired and enjoyed for talent and learning—though we must be allowed to say, that in all that regards the mechanical part of midwifery, he is far inferior to Levret, Baudelocque, the French accoucheurs in general, and we may, we think, safely add his countryman Smellie. We are therefore decidedly of the opinion of Dr. Dewees, touching these Aphorisms, when he says that their utility is very limited or questionable, and their chance of doing mischief great.

After a few preliminary observations, Dr. Dewees proceeds to examine the several parts of the work, and first offers many objections to Dr. Denman's classification of labours, under the four general heads of natural, difficult, preternatural, and anomalous or complex. Though his arguments on this subject we deem conclusive, we shall beg leave to pass them by, in order to present a few of the objections he offers to some of the Aphorisms—referring to the book itself for the full exposition of his views. In section 1, Aphorism 4, Dr. Denman says, "The intention in the use of the forceps is to preserve the lives both of the mother and child, but the necessity for using them must be decided

by the circumstances of the mother alone." To this Dr. Dewees very properly objects, that it would naturally lead the young practitioner, in cases in which the mother herself is in no danger, or can eventually, no matter when, expel the child, not to use the forceps, however important they may be to the preservation of the child: a precept which, if followed, would prove the destruction of very many lives. In Aphorism 5th, Dr. Denman says, "It is meant when the forceps are used, to supply with them the insufficiency or want of labour pain; but so long as the pains continue, we have reason to hope they will produce their effect, and shall be justified in waiting." To this Dr. Dewees answers, that the young accoucheur who is governed by the precept this aphorism inculcates, will be led into an error, on which the life of both mother and child are staked.

"How many cases are there," he says, "where the forceps might not only be used with safety and advantage, but where they are truly indispensable; yet where pains continue, nay, even continue with violence, but unavailingly, either from the bad situation of the head, from the absolute or relative narrowness of the pelvis, or uncommon rigidity of the soft parts: under circumstances like these, we are, through false principles, to subject the woman to all the consequences of the long and violent pressure of the child's head on the soft parts within the pelvis, and thereby hazard inflammation, suppuration or gangrene; while we expose the child to all the evils arising from its head being long and violently compressed by the reiterated contractions of the uterus. Yet here we are forbidden the aid of the only means by which the labour can be terminated with safety to mother and child, or at least, we are told it will be justifiable to wait, maugre the accidents that may ensue."

In Aphorism 14, we are told "the forceps should always be applied over the ear of the child; it must therefore be improper to apply them when we cannot feel an ear." In subscribing to the correctness of the precept, that the forceps should always be applied over the ears of the child, it is very properly objected to the deduction, that it must therefore be improper to apply them when we cannot feel an ear—since there are many cases in which this part of the head cannot be felt, though requiring the aid of these instruments.

"For instance, first, when the head has not completely passed the superior strait, and before it has escaped from the orifice of the uterus, and more especially when this viscus is contracted firmly round the head; secondly, when the head is wedged diagonally in the inferior strait, owing either to the absolute or relative want of size in the pelvis; thirdly, when the head presents originally at the superior strait, with the vertex to the pubes, and the anterior fontanelle or bregma to the sacrum; fourthly, where the anterior fontanelle presents originally at the superior strait, towards the pubes, and the vertex towards the sacrum; fifthly, where either of these presentations have passed the superior strait in the direction just mentioned, and has arrived at the inferior strait; sixthly and lastly, where the vertex or forehead are about to emerge from under the arch of the pubes."

In all these cases, it is impossible to feel an ear by a common examination, (which is our author's criterion of the manageableness of the case)—yet, in either of them the forceps may be exclusively indicated.

Having examined the principal aphorisms which relate to the employment of the forceps, Dr. Dewees next proceeds to those relating to their mode of application. But for his observations on this subject, and for the very excellent rules he lays down for the application of the forceps, we are compelled to refer to the book itself—since to enter into as minute an analysis of them as their excellencies and the important nature of the subject requires, would extend the present article far beyond its proper limits.

Of the cases which Dr. Dewees presents, in proof of the great efficacy of blood-letting in rigidity of the os externum, we shall not here take particular notice, as the practice is much more fully developed and its utility rendered more evident, in an Essay on the means of lessening pain and facilitating certain cases of difficult Parturition, with which he favoured the medical public a few years ago, and which we are aware has had an extensive circulation.

An attempt to explain why more children live that are born at the seventh, than at the eighth months of pregnancy. Discarding all the old theories that were advanced in order to

explain this phenomenon, our author supposes that the causes must be referred to the uterus, and in no respect to the child itself. This, according to him, depends on a greater power and disposition in the body and fundus of the uterus to contract and throw off its contents, at the period of seven than at that of eight months—secondly, on the neck of the uterus being at this period more powerfully constrained to relax or yield to these contractions.

"Until the seventh month," he says, "the body and fundus afford almost exclusively the necessary room for the continually increasing ovum; this happens from, first, the germ being deposited within their cavity, and consequently acting immediately on their fibres, which it distracts in all directions, but more especially in their longitudinal: secondly, from these fibres being longer and more lax than those which compose the neck: the former therefore oppose but little to the ovum, whereas the latter do a great deal. This is a wise provision of nature, otherwise abortion would always take place."

But this disposition to distention has its limits, and the resistance of the neck cannot be maintained beyond the period when the body and fundus are thoroughly developed, which happens commonly at the seventh month. These parts now cannot without injury or pain be stretched any further: they must consequently contract from the stimulus of distention, and the neck of the uterus will be obliged after this period, to afford all the room necessary for the future increase of the fœtus.

"From the balance of power being now in favour of the body and fundus, and this being exerted on the neck, it follows it must either yield, or the uterus must rupture; but as the uterus never has been ruptured in this way, we have a right to infer that there is a disposition in this part to expand when acted upon by them. Now should this disposition be greater than ordinary, or in other words, should the resistance be inferior to the action it has to counteract, the whole neck will be speedily developed and the fœtus will escape through it without difficulty or danger."

From this it is inferred, that we need not be surprised at the frequent occurrence of abortions at this period, as the neck being now compelled to furnish the whole of the required room for the increasing bulk of the ovum, will in con-

sequence be constrained by the powers just mentioned to unfold—which if it does in a just proportion, no evil will ensue : but should this not be the case, and it yields too easily, premature birth will be the consequence. Therefore should the neck of the uterus withstand the influence of the body and fundus, after their first efforts are manifested, he thinks the presumption is, that it will resist it to the full period of gestation.

"Three causes co-operate to this end ; first, the contractile power of the fundus and body will rather be weakened from long distention, and in some measure from their becoming accustomed to its stimulus ; secondly, to this stimulus being rather diminished now, since the ovum does not increase as rapidly as formerly, and to more room being allowed for that increase by the augmentation of the neck ; and thirdly, to the neck being now less passive ; for as it is forced to develope and to augment in width, it becomes incorporated with the body and acts with it, so that when contraction is excited, all the fibres act at one and the same time, and such is the nature of this contraction at the eighth month and at after periods, that it tends to shut the mouth of the uterus ; and hence we see at this period very few spontaneous premature births."

For the reasons just assigned, our author is of opinion that there is no time of gestation at which the uterus yields so resistingly as at the eighth month—and this, he remarks emphatically, accounts for the observation, that eight months' children do not so frequently live as those of seven : for at seven months, the mouth of the uterus will yield to the internal agents, sometimes very readily, consequently the child does not suffer the evils of a long and protracted labour, nor the influence of external agents—whereas, at the eighth month, it almost always requires the concurrence of external causes or violence to throw the uterus into contractions : "hence children of this period seldom live, for they have not only to contend with all the violence that may be offered externally, which is capable of producing uterine contraction, but also, with all the accidents that may arise from a severe and protracted labour, as well as to their being ushered into the world before their final uterine development—we need not wonder, therefore, so few survive these evils."

Reply to Dr. Peachy Harrison's Observations on Impregnation. In his observations, Dr. Harrison attempts to prove that there is a specific sensibility resident in the female genital system, and that this sensibility is seated in the os tincæ chiefly, though also in the uterus itself, in the fallopian tubes and perhaps in the ovaria. He is moreover inclined to think, that the ovaria possess a share of venereal sensibility, "because as venereal desire is first awakened in males by the stimulus of the semen, so it is probable it is first aroused in females by one or more ovula acquiring maturity." "That the venereal sensibility resides in the os tincæ, &c." continues he, "I infer from the structure of the parts, and especially from the projection of the collum uteri into the vagina, where it will of necessity, in the sexual intercourse, receive irritation from the soft and velvet-like head of the penis, well calculated to produce what I have called the venereal orgasm, which consists in a certain excitement of the uterine system, accompanied with exquisite sensations, similar to those which take place in the male at the time of the ejection of the semen, and with *an unknown desire* to receive the venereal stimulus."

We cannot enter here into a minute detail of the arguments adduced by Dr. Dewees, in refutation of the preceding assertions. It will perhaps suffice to call the attention of our readers to the following. It is well known that the venereal desire is awaked in males before the secretion of the semen, and continues to be manifested even after the extirpation of the testes, as is sufficiently proved by the fact that boys at very early periods give evidence of passions of this kind—and that in the eunuch, the horse, and barrow, desire remains. To the female nearly the same observations will apply—girls are known to exhibit many marks of libidinous desire, before we can with propriety suppose "one or more ovula" have acquired maturity—they certainly do not have these desires where we know "one or more ovula" are perfected—and women retain their venereal appetite when we must suppose there is not "one or more ovula" to be matured, namely, after the menses have

ceased to flow—and there are women of very warm passions, and who perhaps have enjoyment from sexual intercourse in its most exalted form, without fecundity.

In respect to the assertion that the os tincæ, in consequence of the irritation from the male organ, becomes the active source of the venereal pleasure, it is remarked, that it seems from the firmness of its texture and its general want of sensibility, but ill calculated for this important office, more especially as all women, as far as we know, have this part, whilst many among them feel no pleasure from the venereal congress, and others experience true disgust—and yet notwithstanding, these women are prolific.

"Now, unless it can be proved that the os tincæ of the woman who feels no pleasure, and that of the one who does, be different, we must conclude, that the os tincæ is not the seat of venereal pleasure." "Besides, women in the latter months of pregnancy, who do feel pleasure from sexual intercourse, have equal enjoyment when the os tincæ is entirely obliterated, or out of the reach of the penis."

Dr. Harrison moreover asserts, that impregnation can never take place unless the venereal orgasm has been excited, and that it will not happen unless the semen is brought in contact with the os tincæ during the venereal congress. These opinions Dr. D. combats by the fact, that many women are perfectly indifferent to the venereal congress—that many women who labour under procidentia uteri have conceived, when the situation of this viscus (their own acknowledgments out of the question,) forbids the idea of pleasure resulting from sexual intercourse—that with other animals, such as the dog, frog, newt, &c. on which the ingenious and accurate Spallanzani experimented, pleasure or venereal contact was not necessary—his syringes were sufficient, and they could hardly have been productive of pleasure: (the same he believes would obtain in women)—that many instances have occurred where the occlusion of the vagina has been such as to prevent most effectually the "velvet-like head of the penis," and the exquisitely sensible os tincæ from coming in contact.

After noticing the above theory of impregnation, our

author proceeds to examine the conjectures respecting superfœtation. But as Dr. H. has offered no very plausible or even lucid explanation of this phenomenon, and as our readers are already acquainted with Dr. D's views of the subject, we shall not engage in the controversy, but proceed to the consideration of sòme of the succeeding essays.

Among these are two which we think merit particular attention. The first is entitled *Account of the use of the volatile tincture of Guaiacum in painful and obstructed Menstruation*—the second "*on Dysmenorrhœa.*" Although situated in different parts of the volume, we shall notice them together, treating as they do of the same subject. We commence with the latter. After premising that dysmenorrhœa had attracted little notice frôm the earlier writers on medicine, Dr. Dewees says:

"Dr. Fothergill, it is true, has vaguely noticed it, and Dr. Cullen has given it *en passant* a place in his 'first lines;' but neither has adverted to its most remarkable attendant, the discharge of a membrane. Dr. Denman appears to be the first to have considered this substance as constituting in part the disease; and has given à short but clear account of the escape of this substance from women who menstruate with difficulty. Morgagni, however, long before him, had given a very remarkable case of this kind, which has not been noticed by the Doctor."

But Dr. Denman, although noticing the formation and escape of this deciduous coat, has not offered a satisfactory explanation of its formation or its consequences. Nor can we regard Dr. Fothergill's theory superior to Dr. Denman's. He has however pointed out one striking feature in this complaint—its disqualifying the woman from bearing children. Having exposed the erroneousness of Denman's and Fothergill's explanations, Dr. Dewees next proceeds to offer one of his own: in doing which, he first notices the history of this complaint—secondly, attempts to explain the formation of the membrane—and thirdly, accounts for the pain which invariably attends its discharge.

"This complaint for the most part commences in women who are obnoxious to it, with the first menstrual periods, and unless prevented, most pertinaciously continues at subsequent returns

of the catamenia. We have never observed any particular constitution or temperament especially liable to it." "The discharge commences sparingly for some time, and is then for a short period, almost altogether arrested; so soon as this happens pain is felt, and this returns and intermits like the pains of labour—after a continuance of these alternate pains for an uncertain period, relief is sometimes suddenly experienced, and there is found discharged from the vagina a membranous substance of uncertain size,—sometimes it resembles, when spread out, the form of the uterus; at other times it is broken into fragments, but always maintains its membranous texture." "Besides the alternate pains which we have just noticed, there is always a distressing aching in the back and hips, and which almost invariably announces the approach of the period; nor does this cease in many instances, until two or three days have elapsed after the catamenial flow."

In accounting for the formation of the membrane, Dr. Dewees enters pretty much at length into the physiology of menstruation, and endeavours to prove that the catamenial discharge is the product of a secretory action in the vessels of the internal surface of the uterus—during its transmission through which, the coagulating lymph of the blood receives a modification which deprives it of the property of coagulating when exposed—and he very judiciously remarks, that this change is a wise and kind provision of nature, by which the female is exempted from the long continued pain and suffering that would necessarily ensue, did not the uterus perform this kind and friendly office.

"Having (we trust) rendered it more than probable, that the fluid thrown out at the menstrual period is the product of a secretory process; and that this process is instituted with the view to deprive the coagulating lymph of the power of coagulation; and that when this secretion is healthily performed, this end is uniformly effected; let us advert to the consequences that would follow, supposing that from some cause or other, an interruption is given to this healthy condition of the uterus: it would seem, under such circumstances, to follow as a consequence, that the fluid discharged would differ from the product of a healthy and well established secretion. The process would be imperfectly performed, and the required changes would not be completely induced, the coagulating lymph would not be entirely deprived of its usual or common capacity, consequently the menstruous fluid would be imperfectly elaborated; so soon then

the fundus is restored to its natural situation. Should we find as this fluid is eliminated from the secretory vessels, it will begin to separate into its constituent parts, the colouring matter will separate from the imperfectly subdued coagulating lymph, and will from its superior density occupy the lower or most depending part of the uterine cavity, and will sooner or later make its escape, while the coagulating lymph will remain either altogether or in part to spread itself over the internal surface of the womb, and will, as it is apt to do when in contact with living parts, quickly assume the appearance and density of a membrane.

"This membrane will be to all intents and purposes an extraneous substance to the uterus, and will consequently stimulate it to the effort of throwing it off, which will be eventually effected by the institution of alternate contractions—and *hence the pain* during this process."

In the essay on the *Volatile tincture of guaiacum*, our author tells us, that supposing dysmenorrhœa to arise from a rheumatic state of the uterus, he was very early led to the use of the medicine just mentioned, and that it has in every instance answered his most sanguine expectations—and at first went even beyond it, since he did not calculate upon any more than relieving the great pain of menstruation—whereas he found that it completely changed the condition of the uterus, and "that this (the pain) was no sooner removed in married women who had hitherto been barren, than they conceived."

He has likewise found, (and his observations have been confirmed by the experience of almost every practitioner in this section of the country,) that the volatile tincture of guaiacum is a powerful remedy in chronic and idiopathic amenorrhœa. For the mode of administering this article in these diseases, we beg to refer to the essay itself.

Essay on the inversion of the uterus. It is here remarked, that many cases are upon record of the complete inversion of the uterus, and its protrusion from the vulva, most of which, as far as his recollection serves him, proved fatal, though no mention is made of death from its being partially inverted, where this viscus was still confined within the cavity of the pelvis. By partial inversion he means, when the fundus of the uterus has passed either through the

os externum, or is turned down inside out, as far as the neck of the viscus.

We are told by our author, that for the inversion to take place, several circumstances must combine—first, the body and neck of the uterus must remain flaccid, and the fundus contract after the expulsion of the child—and secondly, that it is essential the placenta be ingrafted on the fundus—or at least, he has never seen any instance in which the fundus, when prolapsed, was not covered with this mass.

The remote cause of the disease is, whatever may prevent the contraction of the uterus. The proximate, whatever may be capable of drawing the fundus down while the remote cause exists.

"This disease may be suspected when the following circumstances obtain: first, where we find the placenta very bulky and firm at the os externum soon after the expulsion of the child, and that it gives more than ordinary resistance to delivery, when it is attempted by an exertion at the cord: secondly, when we have applied as much force as the cord ought to bear when the placenta is thus low in the vagina, and we do not find it advance; if the patient complain of much pain from this exertion, and more especially when we attempt to aid the force applied at the cord, by hooking the placenta with a finger, we still find it give uncommon resistance: thirdly, when the patient complains of much pain, has some hemorrhage, is very faint, has cold sweats, and becomes extremely pale, more especially when this paleness cannot be accounted for from the quantity of the discharge."

The indications of treatment, consist in reducing the fundus when it has not passed too far through the mouth of the uterus—and when passed too far for restoration, in taking off the stricture occasioned by the mouth through which it has passed, preventing the forcible contraction on the body, and thus producing disturbances and consequences similar to those which arise from a portion of gut being strangulated.

"The first indication is to be fulfilled by placing the back of the fingers against the tumour after the placenta is removed, and pushing it in the direction of the axis of the uterus until the fundus is restored to its natural situation. Should we find

the body of the uterus too flaccid to retain the fundus in its proper place, we ought gently to stimulate it, by rubbing the fingers against it until it contracts sufficiently; nor ought the hand to be withdrawn until this effect is produced."

When alarming hemorrhage attends, it is very properly observed, that we must not depend solely on the presence of the hand to produce contraction, but should give the acetate of lead freely, and have a stream of cold water poured on the abdomen, and more especially make frictions with the bare hand over the region of the uterus. "As soon as the fundus is completely pushed up, and we perceive the uterus to contract, we may safely withdraw the hand."

The second indication is to be met, by grasping the tumour firmly, and drawing it towards the os sacrum pretty forcibly—by this means we make the body of the uterus, the contracted part, pass through its mouth.

The next essay to which we shall direct the attention of our readers, is on *Puerperal convulsions.* We consider it as decidedly the best tract we possess relative to this terrible disease, and one which should be read and studied carefully by all accoucheurs and physicians. It is sufficiently well known to every one at all versed in medical literature, that on no disease of the pregnant or parturient woman, has there existed more confusion and contradiction in authors, in respect to the proposed treatment, than on the one forming the subject of this essay. It is to remove therefore this ambiguity from the treatment, that this sketch was undertaken—and in order to be better able to lay down accurately the rules which should govern our practice in puerperal convulsions, he divides these into three species, namely, the *Epileptic*, *Apoplectic* and *Hysterical.*

We have here an opinion that the causes of convulsions are so completely hidden in obscurity, that, at the present moment, any attempt at a theory of them would not only be futile and objectionable, but might be productive of serious mischief. Such being *his* opinion, whatever may be *ours* on the subject, we shall not engage in an examination

of these causes, or transcribe any of the remarks which are offered upon them. Referring those desirous of being made acquainted with them, to the essay itself, we pass to the characteristic signs of the different species of the disease—and in doing this we beg leave to transcribe the words of our author.

"In the first (Epileptic) we have always, I believe, the premonitory symptoms some days before the attack of convulsions; it is uniformly attended with a strong determination to the head, producing an engorgement of the vessels—it may come on at any period of pregnancy, but most frequently not until some time after the sixth month. This kind almost always produces labour, or at least is almost always accompanied by it, whether as cause or effect I will not pretend to determine—but am inclined to think the former. This kind may terminate favourably when judiciously treated, or may be converted into the second species.

"In the apoplectic species we have nearly all the premonitory symptoms just enumerated, but (they ?) are of much shorter duration. It may attack, like the former, at any period of gestation, but does not necessarily, like it, produce or be (?) accompanied by symptoms of labour. From this it would seem it may be brought on by causes independently of pregnancy, though this process may be considered as an exciting cause; for it sometimes happens, when this is at its height, and is no otherways accessary to it than by the repetition of uterine efforts, the blood is made strongly to determine to the head. It may therefore be either idiopathic or symptomatic.

"In the third or hysterical kind, we have not the same train of premonitory symptoms. If head-ach attend, it is not so severe nor so permanent; there is frequently a ringing in the ears, and it is almost always accompanied with globus hystericus and palpitations of the heart; the face is much less convulsed—the eyes vacillate much less, while the larger muscles of the body are much more powerfully agitated; the patient is sometimes very obstreperous; and the muscles on the posterior part of the body are almost always violently contracted, so much so sometimes, that I have seen a woman raised up in the middle like an arch, while her head and feet, which served to support the body, nearly touched each other. This circumstance I have considered as a very decisive mark of this species of convulsions."

From the view here taken of puerperal convulsions, it will strike every one that each particular species requires

a somewhat different mode of cure, and on this discrimination much of the woman's safety will depend. In order therefore to point out with the greatest precision possible, the method to be pursued in the different cases, our author first lays down the outline of the treatment he has found most serviceable in each species, and subjoins to each division a number of appropriate cases.

"In the (treatment of the) first species," he says, "our great dependance must be placed upon bleeding. This must be done promptly and copiously, or no good can be expected—and one efficient mean to render this serviceable is, that the blood should be subtracted as rapidly as possible in a given time; to ensure this, large veins should be chosen, and large orifices be made. The jugular veins are opened with decided advantage over the veins of the arms, as the blood flows more freely, and is immediately derived from the head, the part more particularly involved in the disease. The drawing of the blood suddenly cannot be too strenuously insisted on, as I am convinced that sometimes on this circumstance alone will depend the success of the operation."

The general bleeding must be aided by topical evacuations, either by cupping or arteriotomy—though we should be attentive not to use these too soon, or in other words, not until the arterial system has been sufficiently reduced by the lancet. Revulsives on the intestinal canal, by means of purgatives and stimulating enemata, and on the skin by blisters and synapisms, are not to be neglected.

Treatment in the Apoplectic Species. The treatment of this species is in nothing different from that recommended for the epileptic kind, except that blood-letting should, if possible, be more promptly employed. It is remarked that it forms a distinction between these two species, that there is one state of the patient in the apoplectic, in which artificial delivery is not to be thought of. In order therefore to state the treatment with the least possible ambiguity, he divides this species into two varieties—the idiopathic and symptomatic. "By the former we wish to be understood, that attack of convulsions in which pregnancy or labour has no agency in the production of: (?) and by the latter, that attack of convulsions which happens during the progress of

labour, but in which this process has no other agency than producing a strong determination to the head." In the first variety we find that the premonitory symptoms precede the attack a short time only—that they are more intense, but that the convulsions are perhaps less severe, though longer and less regular in their return—that the breathing is more strongly stertorous—that no change is made in the os tincæ, and that there is no evidence of uterine contraction. In such cases all efforts at delivery would prove highly detrimental to the patient, and consequently are not to be had recourse to. Our whole duty, under such circumstances, consists in applying the medical treatment proper, were the woman not pregnant. Dr. Dewees remarks that the "rules for the delivery of a patient labouring under convulsions, are simple, clear, and void of all ambiguity—they are these: when there is an evident disposition in the uterus to effect the expulsion of its contents, and then only, we are to attempt to assist it—2d, that this assistance must be given to the efforts of nature with the least possible violence—3d, that unless the labour be far advanced, and the delivery can be very promptly effected either by turning or the forceps, it should not be attempted until we have lessened the danger of a fatal effusion by a copious bleeding—4th, that no attempt should be made to dilate the mouth of the uterus when at all rigid, until we have removed or very much lessened the determination to the head, by a *sufficient loss of blood*—5th, that this *sufficient loss of blood* is only manifested by a cessation, or great abatement of the convulsions, or by an easy dilatability of the os tincæ—6th, that when the former condition obtains, we may safely trust to the efforts of nature to effect the latter, but if it be accompanied by the latter, the more speedily we deliver the patient the better—7th, that turning is the means to be employed when the child is still enveloped in the uterus; but when the head has escaped from this viscus, we must employ the forceps."

Treatment of the Hysterical Convulsions. This species is less dangerous, and much more rare than the preceding.

It may occur at any period of pregnancy without necessarily deranging its economy. Its cure is much more simple and certain than the other kinds, requiring the same treatment as when it happens to women not pregnant. Delivery is not essential to the welfare of the patient, unless the attack occurs at a pretty advanced period of labour, and seems caused by the irritation given by the head of the child suddenly distending the mouth.

"This form of the disease rarely requires more than one bleeding, and that not very large. After we have taken away blood, which should always be done when the pulse is full and tense, we may safely exhibit opium with asafœtida, which will generally pretty speedily arrest the disease. I have never found cupping or blistering necessary; having the bowels opened by injections is important, and enemata are often the best, and sometimes the only method of conveying the remedies just mentioned."

We shall here close this analysis, and be forced to pass by unnoticed a few essays and some highly interesting cases. The answer to Dr. Harrison's reply being merely controversial, and containing in fact nothing more than what we have already seen in the former paper, must not engage our attention. Dr. Dewees's "*Observations on part of Burn's history of the Gravid Uterus*," his "*Observations of Mr. Fogo's paper on the importance of the Uterus*," and "*Observations of Mr. Charles Bell's paper on the Muscularity of the Uterus*," are productions well deserving the attention of physicians and physiologists, and may, we think, be offered as good specimens of the soundness of our author's powers of reasoning, and of his intimate acquaintance with the subject on which he writes. In respect to the "*Essays on the Rupture of the Uterus*," to the "*Observations on Retroversion of the Uterus*," "*Strictures upon Dr. Merriman's opinion of Retroversion of the Uterus and extra uterine Conception*," and the "*Essay on Uterine Hemorrhagy*," we would very particularly direct the attention of the profession, as being in our opinion decidedly the best productions we possess on these subjects. Being however published in former numbers of this Journal, we trust that *all* our

readers will refer to the essays themselves, and not rest contented with any analysis of them we could offer, in so limited a space, as that which this already too extensive paper would allow us to devote to their examination.

Art. XII. *An Exposition of the danger of Interment in Cities; illustrated by an account of the Funeral Rites and Customs of the Hebrews, Greeks, Romans, and primitive Christians; by ancient and modern ecclesiastical canons, civil statutes and municipal regulations; and by chemical and physical principles. Chiefly from the works of Vicq D'Azyr, of France, and Prof. Scipione Piattoli, of Modena; with additions.* By Felix Pascalis, M. D. New York. 1823.

In every country, an imperious duty devolves on public authorities, and those who by their profession are more particularly the guardians of health, to seek out all causes which may tend to the deterioration of the health, or endanger the lives of their fellow-citizens, in order, if possible, to remove them altogether, or at least to apply such means as will be best calculated to limit their sphere of action, or render them less noxious to the human system. With this in view, some of the municipal authorities, and many of the most eminent physicians of New-York, struck with the frequent visitations of pestilential diseases in that city, and sensible of the very bad condition of its burial grounds, were led to consider such places as exercising a considerable share in the production of these maladies, and to devise the most appropriate means of correcting their baneful influence. As early as the year 1806, Dr. Edward Miller, John Pintard, Esq. and Mr. Winant Van Zandt, were appointed by the board of health of New-York, a committee to report on measures necessary to secure the health of that city. After mature deliberation, and a careful investigation of the subject, the eminent physician whose

ıame we have mentioned, reported that "interments of lead bodies within the city ought to be prohibited." But his advice, however important it might be, seems to have net with so little attention from the public officers of New-York, that Dr. F. Pascalis was induced to publish in the Commercial Advertiser for August 17th, 1822, some strong ıdmonitory remarks on the insalubrious nature of the nume-rous places of interment of that city. These, or perhaps other causes, finally producing a conviction of the real dangers attending the too great vicinity of such receptacles of the dead, led the board of health, and other authorities to prohibit, at least during the hottest months of summer, city interments. It is for the purpose of enforcing the admonitions he had given, and acquainting his "fellow-citizens with the dangerous consequences of city interments, and of illustrating the subject by references to historical facts, civil and religious statutes, and to results drawn from the principles of natural philosophy," that our author has undertaken the work before us.

It is principally made up of chapters from the works of Vicq D'Azyr and Prof. Scipione Piattoli. But Dr. Pascalis is entitled to more credit than is generally allowed to translators. He has arranged and condensed both the works we have named, so as to suit better the present circumstances, and has added matter of his own.

"The first chapters of the Exposition are condensed from a long dissertation on ancient modes of burial. The ninth was partly abstracted from the preliminary discourse of Vicq D'Azyr. In the tenth are assembled together the legal acts scattered throughout the text of both writers. The eleventh is abridged from Piattoli. While I cannot trace," he continues, "an exact line between the translated and annexed matter in the work, I may say that for the authorship of the four last chapters, I alone am responsible."

As our object in framing this article is more to direct the attention of our readers to the *subject* of the work before us, and to recommend it to their serious consideration, than to give a minute and critical analysis of its various chapters, (which from our limits would be very imperfect,)

we shall endeavour to present an outline of its contents, and point out those parts we deem most interesting and useful.

An attentive investigation of the mode of burial or disposal of the dead in ancient times, will serve to convince us that the necessity of interment at a distance from inhabited places, founded as it is on the pernicious nature of the exhalations produced by animal decomposition, has been acknowledged by the general opinion of the most celebrated nations of antiquity, and sanctioned by religious tenets in every age. Among ancient nations, however, little advanced beyond the infancy of civilization, or among barbarous tribes, the modes of burial or destruction of the remains of the dead were shockingly cruel, absurd or contradictory, and well calculated to illustrate the superstitions of the times. Thus according to Herodotus, Cicero, Lucian and others, some Asiatic nations after butchering the aged, devoured their limbs: they murdered the sick, and feasted on their entrails. The inhabitants of the shores of the Caspian sea, and also the Bactrians, exposed the dead to the ferocious animals of the neighboring forests—others, as the inhabitants of Colchis, threw them into lakes—and the Assyrians into marshes—the Scythians buried them in snow—as do also the inhabitants of Laponia, of Siberia and Kamtschatka. When forests afforded them fuel, the survivors disposed of the dead by means of fire: such was the custom among the Germans, Gauls, the inhabitants of Lithuania, the Tyrians and Phrygians: the Ethiopians, Goths, left them in the open air—the Caramantians covered them with sand, and the inhabitants of Scio threw them into the sea. But, as Piattoli remarks, neither snows, forests, nor even the seas afforded, in all instances, a sepulchre for all the dead—the earth alone could, in all places, be adequate to that purpose: in consequence of which, we find that the most ancient and common custom, was to deposit the dead in her bosom, which is called *inhumation*. The history of the formation of man, as well as religious traditions, served to consecrate this custom. It appeared just, to restore

human bodies to the common parent from which they were thought to have originated. With the progress of time, however, this very wise custom, the result of reason, received from the passions of men sensible modifications. From the horror with which they contemplated the termination of life —the mortifying thought of being forgotten for ever, and also the desire of offering resistance to that constant revolution by which all beings are destroyed—arose that extraordinary mixture of funeral ceremonies described by historians, and of which philosophers penetrate the aim and object.

The custom of keeping the dead as long as possible, was soon adopted, either in the hope of beholding them return to life, or because they could not be made to part with the remains of those they had loved. But in a more advanced stage of society, the voice of religion united to that of nature and policy, called for the careful interment of the dead. The Egyptians more particularly, attached to burial an idea of honour—considering it as a recompense for virtue, and an incentive to public emulation. Religion then giving the consoling prospects of a future life, where the soul should still preserve recollection of its earthly existence, excited a respect for the tombs of those who had lived virtuously. Among their many customs, we find that they built no houses, erected no walls, nor constructed temples on ground that had been used for inhumation—a precaution that tended to separate widely the dead from the living, and to restrict interments to distant situations.

"The Assyrians, Medes, Parthians, Tyrians, Phenicians, Ethiopians, the Egyptians also, and the Persians, had always vaults in particular places destined for interments. The Chinese and the Peruvians, situated on the opposite sides of the globe, had the same practice. There are tombs of kings and great men of distant ages which are excavations in rocks, upon the most solitary mountains. Gyges, king of Lydia, was buried at the foot of mount Tmolas. The remains of the kings of Persia were entombed on the royal mountains near the town of Persepolis; Silvius Aventinus was buried on the hill that bears his name, and king Dercennus was interred *within* a high mountain, as Virgil attests.

"The ancient Russians transported the bodies of their deceased princes to the deep caverns along the Boristhenes; travellers, led by curiosity, still visit them. The Danes constructed artificial hills for the sepulchres of their kings."

We are indebted to the researches of several travellers, though more particularly of Lechevallier, for the discovery of similar mounts near the borders of the Hellespont, which in all probability cover the remains of Ajax, Hector, Achilles, and his faithful friend Patrocles.

The Chinese place their tombs beyond the walls of cities, and usually on hills covered with pine and cedar trees. They cover the coffin with earth to the height of from six to ten feet, in the form of a pyramid. In the kingdom of Siam, ashes are deposited under similar pyramids, erected around temples. In the island of Ceylon the poor inhume their dead in the midst of the forests, whilst the rich burn theirs with much pomp. The Turks bury indifferently their dead beyond and in the cities, and even in their mosqués. Among the Hottentots the place of inhumation is either an excavation in the rocks, or a cavern. The Indians of North America observe the same salutary custom of interring their dead beyond the limits of their villages. The inhabitants of Greenland, as Crantz assures us, place their tombs at a distance from their habitations, and in an elevated situation.

From a desire to replace the loss of a beloved object, by an attempt to trace an outline of features vividly impressed on the imagination, the art of delineation probably arose. Yet no sooner was this accomplished, than man, not satisfied with portraits, busts or medals, wished to retain the very object itself—and to this wish we must trace the invention of the art of securing a kind of perpetuity to the inanimate form.

The Egyptians therefore "embalmed the dead by drying, salting, varnishing with wax, smearing with honey and cedar dust, and by using every art capable of preventing the action of the air upon the stagnant humours, to preserve the body from corruption, and to secure it in such a manner that it might be kept without danger to the living."

The self love of individuals, and the peculiar notions of

the times, in relation to the union of body and soul, gave great encouragement to this art, and in consequence, the custom became of such general prevalence, that being regarded by public authorities as dangerous to the health of the community, it was finally abolished.

"At first the bodies embalmed were kept in distant districts, in vessels of glass or clay, placed in some isolated cave, or in dry sand, or under sand-stone impervious to water. But these customs at length degenerated so much, that houses were filled with these vessels; they were kept as a most sacred civil pledge. This superstitious practice, however, could only have been prevalent among the great and the rich. The people, that is the greater number, were contented with simple inhumation; and there have been whole nations among whom burial was practised generally and without interruption."

Before a considerable time had elapsed, the Egyptians entertaining the conviction that to the *embalmed*, was to be traced the origin of the many contagious and mortal diseases that desolated their country, removed them far from all inhabited spots. Soon after, and for similar reasons, the practice of burning the bodies of the dead, and preserving their ashes was introduced, and tombs and vases were only used for the ashes of the funeral pile. Even these ashes, venerated as they were, were excluded from within the walls and precincts of cities, and the urns in which they were contained, deposited in places exclusively consecrated to burial. The highways were for a time bordered with tombs, and slabs of marble covered with inscriptions.

"Then religion introduced new dogmas that favoured this custom. Philosophy theorized on the nature of spirits and the activity of fire, which, it said, would the most promptly disengage the soul from its cumbrous prison, purify it by delivering it from the burthen of a perishable body, and rapidly elevate and unite it to the soul of the universe. The industry of the Egyptians discovered a new means of preserving the ashes of the dead by the use of the incombustible amianthus. From the expensive nature, however, of the funeral pile, with its accompaniments of rich drugs, spices and perfumes, we presume that the people in general never obtained that distinction.

"If we search through history we will find that soldiers have

been mostly employed to construct roads; and that subterranean cavities and excavations have always been made at a distance from cities. It is also certain that in several countries there have been public funds assigned for the erection of tombs, and for the maintenance of pyres that were in constant requisition in very populous states. Among so many customs, produced by caprice and the love of change under different circumstances, the natural sense of man, his religious doctrines and his laws, have ever agreed in removing the dead from the living; and the motive for which tombs were located at a distance from cities, has been always kept in view."

The limits to which we are forced to restrict the present article, will not permit us to devote much room to the subject of the funeral "Rites among the three most interesting nations in history, the Jews or Hebrews, the Greeks and the Romans." It is however one of very considerable importance, and well worthy the attention of the philosophical physician. Contenting ourselves, for the present, with a brief account of these rites, we would earnestly recommend such of our readers as are anxious to obtain information on the subject, and to trace to their true origin the elements of our own customs respecting funeral ceremonies, to the volume under review. By perusing it, they will find that the traces of Judaic antiquity, preserved perfectly pure and inviolate, lead back to the most ancient times in which inhumation was a general practice—and that the first example of it must be referred to the time of Cain, who having raised his hand against his brother, thought to conceal the deed by covering the body of his victim with earth. So early as the time of Abraham, bodies were buried at a distance from the habitations of the living. From the Holy volume we learn, that the patriarchs, Sarah, Isaac, Rebecca and Leah were deposited in the cave Hedron, where, according to many commentators, the remains of *all* the patriarchs were afterwards assembled: that the tomb of Rachel was situated aside of the road from Jerusalem to Ephrata—that during the Egyptian captivity the tombs of tbe Israelites were placed in some distant spot, according to the custom of the people in whose country they were: that Moses, by the

command of God, was interred in the valley of Moab—Miriam, his sister, at Kadesh—Aaron at Hor, and Eleazer, the son of the latter, as well as Joshua, on the mountains of Ephraim—all tending to shew that inhumation of the dead near the habitations of the living, was not allowed.

" After the entrance of the Jews into the promised land, the establishment of the Judaic law, and the inauguration of the religious ceremonies it prescribed, they found that the commands of God forbade them to allow the dangerous vicinity of the dead."

They were, however, permitted to inter at their country seats, and here was displayed all the luxury of the grandees of the nation. The nurse of Rebecca and Deborah, and the unfortunate Saul, were buried at the foot of a tree. Vaults dug in the hill of Zion, under the foundation of the temple and in the royal gardens, were destined for the last abodes of the kings of Judah. Amid all the vicissitudes which this people underwent, but few foreign customs, such as burning and embalming the dead, were introduced among them. In Paralipomenes, and the books of Jeremiah, the ceremony of burning is mentioned as a rite introduced in favour of the kings only. The bodies of Saul and Jonathan were burnt to ashes by the people of Jabesh-gilead, to protect them from the insults of the Philistines. We therefore see that in the earliest times, caverns and fields were always the places appropriated for interments. Elijah was inhumed in a grotto where other bodies also were placed, among which was one that, according to the Holy Scriptures, miraculously recovered life as it touched the bones of the prophet. In the progress of time, " Each town always had a public cemetery beyond the walls. Some think that that of Jerusalem was in the valley of Cedron, near which the Pharisees bought the field of Vasaja as a burial ground for strangers. A custom so steadily maintained, and faithfully pursued by a people who adopted it in obedience to the commands of God, should be an example of authority to Christians "

At a very early stage in their history, the Greeks adopted the custom of burning their dead. From Homer we learn

that the bodies of their great captains and of their sovereigns, were abandoned to the flames, and the ashes preserved in magnificent urns. Achilles, Patrocles, Ajax, were treated in this way, and piles of earth formed over their remains. Philoctetes burnt the body of Hercules by express command of his hero. Nevertheless, inhumation was an early custom among the Greeks, and this mode of disposing of the dead was more universal in their country than any where else:

"And then, the salutary custom of committing the body to the earth beyond the limits of the cities, was never infringed. The Thebans, the Sicyons, the people of Delos and Megara, the Macedonians, the inhabitants of Chersonesus, and of almost all Greece, had the same customs in this respect. The most celebrated legislators made it an interesting point of their codes. Cecrops of Athens, wished that the dead should be transported beyond the walls; Solon adopted, and re-established this wise regulation in all its vigour; and there was not at Athens, until the latter days of the Republic, but a small number of persons buried within the city; and this had only been allowed as an honourable distinction, in favour of some heroes. It was thus that they buried in the public walk of the Ceramicus, the bodies of those who had died in defence of their country. Plato in his republic, would not even permit inhumation in fields fit for cultivation, but reserved for that purpose sandy, arid ground, that could not be applied to agricultural uses."

The same laws were enforced in Grecia Magna. The Carthagenians found outside of Syracuse and of Agrigentum, the tombs of the inhabitants of these cities. The Tarentines followed the same custom: indeed the "whole religious doctrine and mythology of the Grecians, tended to support the laws that removed the bodies of the dead completely from the habitations of the living."

Among the Romans, the faithful imitators of the Greeks, the same customs, respecting the interment of the dead, seem to have prevailed—and all inquiries into their causes and practical application, will show that they were occasioned and sanctioned, by a wish for the promotion of the public welfare, and the preservation of health. With the exception, therefore, of Valerius Publicola and Tubertus, who obtained the honours of interment in Rome—of the posterity of the former to which this honour extended, and the vestal

virgins who enjoyed singular honours and privileges, it does not appear that ancient Rome permitted the interments of any of her citizens within her walls. In the course of time, however, military characters enjoyed the same privileges, and ambition and vanity finally rendered it common among the nobility of the nation. The law of the twelve tables, consisting of a collection of the institutes of the Greeks, or rather of a result of certain researches into the ancient rights of Italy, by prohibiting expressly the burning or inhumation of any body in the city, merely renewed a custom which had prevailed from time immemorial, with occasional interruptions.

The burning of the dead was introduced into Rome, from the impossibility of committing to the earth all the bodies exposed throughout the territory, during the many wars of the republic, and the incursions of the barbarians —a neglect which would have seemed impious to the superstitious Romans.

"But we suppress many details in relation to the burials of the Romans, which only serve to prove that, in concert with the Egyptians and Grecians, their predecessors and contemporaries, they held every thing sacred and inviolable that had any reference to the dead; and felt, besides, a conviction of the danger and impropriety of crowding the dead into places of deposite among their habitations and temples; or as one of the commentators of their laws eloquently expresses himself on the subject of the law of the twelve tables—*Corpus in civitatem inferri non licet, ne funestentur sacra civitatis.* The emperors Diocletian and Maximian enacted the same prohibition in the Code, *lex.* 12, on the places consecrated to the worship of the gods, *Ne sanctum municipiorum jus polluatur.* (*Paulus*, lib. *Sentent. Tit.* 21. s. 2.)"

Among the early Christians, the rich only adopted the custom of burning the dead—and burial beyond the precincts of cities was, with the exception of a few, equally obligatory on all. Exceptions were found among those who had died invested with some high station or dignity. In the times of persecution, those who had embraced Christianity, in imitation of the Jews, who erected synagogues and chapels near the tombs of those remarkable for

virtuous lives, and went thither for prayer—in imitation likewise of the Greeks, who offered sacrifices beside graves, and of the Romans who constructed over the *hipogœa*, halls where they assembled to render the last services to the dead—in imitation of these, we repeat, the early christians built over the catacombs, those retreats so venerated by the lovers of antiquity, to which they resorted in crowds to perform the mysteries of their religion, and to attend the *agapes*, or collations provided at funerals. But even this eagerness for the preservation of remains, did not prevent them from endeavouring to anticipate the mischief likely to result from the vicinity of these bodies to the places of assemblage and worship—and hence they carefully filled with earth whatever places were left empty in different parts of the catacombs.

But owing to the increased number of martyrs, such places of burial were no longer sufficient to contain their remains, and in consequence, some citizens of Rome, who had joined the church, appropriated their wealth and lands to forming burial grounds; and many patricians and Roman ladies also offered extensive tracts for the same pious purpose. Thus originated christian cemeteries.

"The law of the twelve tables had, by the gradual infringements of the great, become nearly obliterated, till it was restored by Adrian to its original vigour; and Antoninus Pius extended it to the whole empire. A new law, or a law just renewed, is always strictly observed. The dead must therefore have been then transported out of the city; but they soon began to evade the regulation, and one hundred and fifty years after, Diocletian and Maximian were obliged to strengthen it by new decrees."

In this situation things remained, until peace was restored to the church by the conversion of the emperor Constantine. The temples of the idols which already were out of favour and scarcely resorted to, were now purified and transferred to the worship of the true God. The same altars on which the holy mysteries had been celebrated in the obscurity of the catacombs, or cemeteries, were transported into the cities—the people ornamented the places selected for

interments with great care, and each of them at length became the sites of consecrated temples.

"The wish of retaining the dead within cities seemed to increase even by impediments. It became regarded as an enviable privilege, to be allowed to occupy after death the places where holy persons had been in the habit of offering their prayers to Heaven. And at length they carried their respect so far as to believe that there were emanations from the bodies of saints, of power to warm the hearts of the devotional, and to yield impressions capable of disposing to fervour and piety."

In this way the custom began to extend. Constantine, in testimony of the highest honour and distinction, was allowed interment in the vestibule of the Holy Apostles in Constantinople—a favour which added new lustre to the supreme dignity of the greatest earthly potentate. Several of his successors who had been patrons of the church were distinguished in like manner: gradually the same honour was granted to founders and benefactors of churches—afterwards extended to bishops and priests, and finally to the laity, who, having no other titles to obtain it, offered large gifts to the church, and distributed alms liberally. In a word, the abuse was finally carried so far that pagans, christians, the impious, and those who had led a holy life—all were indiscriminately allowed interments in the interior of temples.

Nevertheless, at this period, the public burial grounds were still located in distant situations: and the number of those allowed city interments was as yet inconsiderable. But gradually it increased, until finally interments in Constantinople and other cities of the empire became general, and it required all the authority of Theodosius the Great, and the emperors Gratian and Valentinian II. to put a stop to the practice. In renewing the edicts of his predecessors, and in publishing the famous constitution found in the code that bears his name, the emperor Theodosius was, like them, actuated by the desire of preventing the infection of the atmosphere, by the dead being, as it were, heaped in crowds among the living. This prohibition was soon in force over the whole Roman empire, and was respected for a long time, either from the veneration in which this great

prince was held, or because his descendants contributed all their efforts to preserve the full and entire execution of the ordinance.

"These observations led to an important reflection. However the ideas of the Pagans and Christians may have differed with regard to a future state, however dissimilar may have been the customs, practices and ceremonies introduced into the christian church, still the most enlightened sovereigns have maintained or established laws, which had for their object the preservation of the public health from the dangers of indiscriminate interment. The primitive canons, the bulls of the popes, and the authority of tradition concurred to relieve cities from the fatal presence of the dead. But superstitions daily arose that blinded the pious to their danger; and the flattering hope of participating in the merits of the just, by being consigned to the dust which had been consecrated by their ashes, and the honour it was to have been judged worthy of this favour, warmed the religious zeal of some, and excited the self-love of others, until the reigning custom was a total breach of the law. An honour, once the highest earthly distinction, and reserved for emperors, became the portion of the lowest citizen, or rather a right common to all equally."

After the sixth century, synods as well as councils, taking into consideration the abuses of city interments, endeavoured to abolish them, and to restore in all its vigour the ancient discipline of the church. The Council of Bracar, held at Brague in the year 563, instituted a memorable canon, by which not only interment in churches was prohibited, but also cities were declared to have a right to prevent burial within their limits. From this time until the eighteenth century, the very many councils holden in different parts of the christian world, directed their attention to the subject of interments, and all, from a firm conviction of the dangers of allowing proximity of the dead to the living, united in prohibiting the burial of bodies either in churches or within the walls of cities.

"May we not conclude from all these authorities, says Piattoli, that the actual custom of burying within and under churches should be proscribed as contrary to the spirit of our religion?"

The seventh chapter of the volume before us contains the

very eloquent and celebrated ordinance of the archbishop of Toulouse concerning interments in churches. It was written from an intimate conviction of the dangers attending such interments, and from representations made to him by the clergy of the metropolitan church of that city, that in violation of the holy canons, interments in that church had increased exceedingly, and that the air had become sensibly contaminated by fetid exhalations from vaults which were not deep, and were continually re-opened for the admission of fresh bodies. The succeeding chapter presents us with the decree of the parliament of Paris in 1765, against the abuses of interments. In the preamble of this decree it is asserted, that "daily complaints are made of the infectious effects of the parish cemeteries, especially when the heats of summer have increased the exhalations; then the air is so corrupted, that the most necessary aliments will only keep a few hours in the neighbouring houses."—The provisions of the act, in nineteen articles, are absolute, and admit of no exceptions. It ordained that all cemeteries and church-yards in the city of Paris were to be closed, and remain unoccupied for the space of five years, or longer if thought necessary by the proper officers and physicians; regulating also, the establishment of eight cemeteries at a distance from the suburbs, and the means to facilitate the transportation of bodies to these places. By this decree it was thought that the city of Paris would be sufficiently protected against the danger of cemeteries and church vaults; yet eleven years after, (1774) the same authorities were obliged to make another ordinance against the opening of vaults for the admission of bodies. These acts being in accordance with public opinion, prepared the way for an universal reformation of the abuses of interments; and meeting with the approbation of Louis XV. led this monarch to grant to the parish of St. Louis, at Versailles, three thousand six hundred square feet of land at a distance from the city, to be used as a cemetery in the place of the old one. Taking a still more active part, he issued a royal declaration in March 1776, the articles of which "prohibit grave-yards

in cities or towns, of which the archbishop of Toulouse had already given an example in his diocess ; and they permit no interment in churches, chapels or cloisters, but for such persons as have already been mentioned in speaking of the decree of the parliament ; and ordain, besides, that even those shall not be interred except under vaults covering a space of seventy-two square feet, built of stone and flagged; the bodies to be placed six feet deep in the earth under the lower pavement of the vault: they also invest municipal corporations with the right to obtain and hold in fee simple any grounds for new cemeteries. Thus all that could be devised in point of legislation, to do away the evils of interment in churches and towns were at length accomplished."

About the same period, as we are informed by Fodéré, (vol. vi. p. 296) the court of Turin made the same prohibition, and ordered the establishment of cemeteries at a distance from the city. The French legislators, adds Fodéré, have strictly enforced the execution of this law; and in the summer of 1810, Mr. De Circé, archbishop of Aix, having solicited, whilst on his death bed, the privilege of being interred in his cathedral church at Aix, the government obstinately refused to grant it. At Vienna there were no cemeteries around the churches; the empress Maria Theresa had a public one established beyond the capital.

The succeeding chapter is entitled "Medical Inquiries into the Dangers of Interments." It contains an analysis by Vicq D'Azyr, from which it is principally taken, of the various writings on the subject previous to the appearance of professor Piattoli's learned work. We shall not follow the author into the examination of all these works, as it would occupy more time than we can well devote to this article—but merely present a few of the most striking facts of the dangerous effects of the inhumation of the dead within cities, and likewise offer some of the remarks by which they are accompanied. The first writer mentioned by Vicq D'Azyr is Dr. Haguenot, a professor in the university of Montpellier. He relates, that—

"On the 17th of August, 1744, towards evening, the body of a layman was conveyed to a vault in the parish of Notre Dame in Montpellier, attended by a numerous procession of the clergy and laity. No less than three men perished suddenly on this occasion; a fourth was with difficulty recovered from a state of asphyxiation; and a fifth was attacked with severe and alarming symptoms, which left him for a long while pale and feeble, and his recovery was, very properly, termed a resurrection. The catastrophe took place in the following manner:—While lowering the corpse, a man first went down to support the coffin, and fell senseless; another followed to assist him, and though drawn out in time was afflicted with the severe illness just mentioned; the third, who had courage to proffer his services, descended with a rope around his waist, and had he not been drawn up immediately, would inevitably have died; and a fourth, a strong and vigorous man, trusting to his robust constitution, and only hearkening to the call of humanity, dared the danger, and died as he had entered the vault; the fifth came out once to recover strength, and returned the second time, staggered from the ladder, and fell dead. The bodies were at last drawn up with hooks. Such a tragical event filled all Montpellier with dismay; and the intendant, Mons. Le Nain, ordered it to be investigated."

Haguenot was commissioned for this purpose—and having had the vault re-opened, he made some experiments, which are detailed in the work before us, but which, as they were made at a period when chemistry was yet in its infancy, and consequently are very defective, we shall not notice here. Upon this subject the translator confines his remarks to two essential points—namely,

"That the decomposition of the bodies in the vault was rendered the more active by the temperature of the month of August, in a climate and latitude exactly similar to our own; and the presence of the external air assisted the extrication of the gases, and the elective affinities of *carbon*, *oxygen*, *hydrogen*, *phosphorus* and *nitrogen*, and *volatile* and *fetid* oils connected with those elements, all deleterious or deadly."

In order to illustrate further the dangers attending improper interments, Dr. Haguenot remarks, that besides the terrible effects of these vapours when mixed with atmospheric air, their more gradual or weaker evolution fills it with deleterious qualities, the germs of fatal or epidemic ma-

ladies, or they aggravate the symptoms of those already existing.

"In the neighbourhood of the church where the above calamity took place, the small-pox broke out and raged with great violence."

He also regards it as an error to suppose that the stones and mortar of a vault can confine such gases entirely; and as dangerous to trust to the small number of dead bodies within them. The force of these gases, he remarks, depending in a great measure upon their being pent up as they are formed—a single body may cause all the mischief, when reduced to a perfect state of putrefaction.

The next author noticed by Vicq D'Azyr is Maret, a celebrated physician of Dijon, who published his work in 1773.

"He, still more strongly than Haguenot, confirms the theory of epidemic diseases being engendered or aggravated by the impure emanations from graves and vaults. A simple catarrhal affection or influenza existed in Saulieu, a populous town of Burgundy. Two persons who died with it were buried beside each other, in graves dug under the pavement of the parish church of St. Saturnine, within an interval of twenty-three days. It appears that the coffin of the first, a very large subject, accidentally broke, and a quantity of putrid fluid was effused, which in an instant filled the whole building with a stench intolerable to the by-standers; out of one hundred and seventy of these, no less than one hundred and forty were seized with a putrid malignant fever, which took the symptoms of the prevailing influenza, differing only in intensity and fatality, which was evidently caused by this temporary diffusion of impure air."

Vicq D'Azyr, after giving an account of several other writers on the same subject, dwells upon the work of Navier, who wrote in 1775, confining his remarks more particularly to cemeteries. Navier states, that the confidence with which they are suffered to exist in cities, is founded on the erroneous belief that bodies in the earth are very soon destroyed. So far from this being the case, he ascertained that four years are not sufficient for this purpose; and relates,

"That having examined three bodies disinterred, the one after twenty, the second after eleven, the third after seven years, he found the bones were still invested with some flesh and integuments; from which it is certain that whatever receptacles of the dead are open, there is unavoidably a contamination of the air, or some attack of disease occasioned or increased: this, he says, he has often witnessed."

He very properly exclaims against the practice of perpetuating cemeteries by means of charnel houses, to which the remains of carcases, and bones still covered with flesh, are transported. He lays it down as a maxim that no old cemetery should be disturbed under ten years, in order to give full completion to the work of decomposition. He directs powdered quick lime to be thrown over the coffin when placed in the earth, as counteracting the gases generated within. He disapproves of the custom of planting trees in grave-yards, because the roots occasion much trouble to the diggers, and sometimes break or loosen the soil so much as to open a free passage for effluvia; and as their branches obstruct the free dispersion of the air charged with mischievous vapours. He further wishes cemeteries to be open to every point of the compass.

"Such important truths as these," says the translator, "when skilfully inculcated in other nations, have occasioned an almost total change in the location of places of interment. They may still serve to convince the prejudiced, satisfy the timid, and reconcile the mistakenly zealous averse to the proposed reform."

The eleventh chapter is abridged from Piattoli, and consists in an attempt by that author to establish, by "facts and observations," the pernicious effects of animal decomposition. It is an exceedingly interesting article—replete with sound remarks and conclusive facts—presenting at one view the opinions on the subject of some of the most celebrated names in the annals of history, philosophy and medicine.

"Diodorus of Sicily," says the author, "speaks of pestilences that were produced by the putrefaction of animal substances. St. Augustin makes mention of a great number of animals having been thrown upon the shore, where, by their decay, they caused an extensive plague. Egypt is ravaged almost every year by ma-

lignant fevers; and from that country the small-pox has spread over all the earth. The waters of the Nile, say some writers, leave with their slime an immense number of aquatic insects, which, as they corrupt, fill the air with pestilential miasmata. (*Vid. Mead on the Plague.*) Forestus (*L.* vi. *Observ.* 9) and John Wolf (*Rer. Mem. vol.* i. *cent.* 10) relate that some fish thrown dead upon the shore, occasioned a great mortality. The swarms of dead locusts in Ethiopia generally create epidemic diseases. Those on the sea coast suffer much from the putrefaction of whales thrown upon the beach.—(*Diemerbr. de Pest. l.* i. *c.* 8.) Paré tells us that in his time a circumstance of this nature produced an epidemic plague in Tuscany. And Lancisi writes, that the exhalations from a dead ox, killed an unfortunate traveller in the neighbourhood of Pessara, (*De Bovill. Pest. p.* 1.) Lucan speaks of the ravages of an epidemic in the army of Pompey, near Durazzo; it was caused by the carcases of horses killed and left upon the field. Ammianus Marcellinus informs us, that the camp of Constantine the Great was desolated, in consequence of the same imprudence."—" Almost all long sieges, in which much blood is shed, are accompanied by fevers and mortal diseases. The war of the Swedes in the seventeenth century, occasioned a great pestilence in Poland. Long and obstinate wars had the same effect in Austria, in Syria, and in many other countries; and the same happened frequently in many parts of Asia. Paré relates, that in 1562, a pestilential fever spread for a circumference of ten leagues around Guienne: it was caused by the putrid exhalations of a pit filled with dead bodies two months before."

Piattoli further states that emanations of this kind are very penetrating, and not only *produce* violent diseases, but render dangerous those which have already supervened, or to which persons are predisposed. Headaches, attacks of fever, nervous disorders, convulsions, and even miscarriages are sometimes the consequence.

"Putrid and malignant fevers," he says, "and periodical diseases often make their appearance in populous cities without any apparent cause. May not their cause, known to us only by its fatal results, be the common practice of interments in the very midst of our dwellings? We have said enough in demonstration of the indispensable necessity of placing public cemeteries beyond cities, to justify the wise dispositions of the administration on this subject, and to destroy completely those prejudices which had taken root in the credulity of the public—prejudices diametrically opposite to the interests of those who disseminate them, and who would cast them aside were they

nore enlightened or more able to calculate and foresee what is letrimental to the health of their fellow citizens."

Such is the opinion of Piattoli, and Vicq D'Azyr, on the langers attending animal decomposition, and such we must iere state is our own belief. We are perfectly aware that y many members of our profession a different opinion is naintained, and that by them the gases which result from he decomposition of animal bodies are considered as in-iocuous, or at least as inadequate to the production of hose dangerous effects we have adverted to in the prece-ling pages. But were not the facts already adduced suffi-ient of themselves to refute such an opinion, they should ear in mind that, as Dr. Pascalis has well remarked, in his dditional chapter on the causes that infect the atmosphere, oth vegetable and animal substances, with heat and mois-ure at the temperature of 72°, are converted in a little time nto a great variety of gases, *carbonous*, *carbonic acid*, *car-buretted*, *sulphuretted*, *phosphuretted*, *hydrogenous gases*, with nuch *azote* or *nitrogen*, and more or less *oxygen*—all of which are morbid, or deleterious. Apprized of these circum-stances, (and every physician ought of necessity to be ac-quainted with them,) it is difficult to conceive how they should in any respect be opposed to the opinion that recep-acles of the dead, in the midst of populous cities, are un-iealthy, and become often the source of wide spreading liseases—and how all the members of the medical profes-sion, at all gifted with feelings of humanity, should not inite with our authors, their learned translator and his iumble reviewer, in reprobating the pernicious custom, and soliciting its entire abolition. Already a very celebrated rofessor in a neighbouring college, although he does not consider it indispensably necessary either for the comfort or health of the inhabitants, that the custom of city inter-ment be altogether prohibited, recommends that they should be exclusively confined to public and private vaults, and that no grave ought to be permitted under any circum-stances, "for such is the loose texture of the soil in grave-yards where this mode of burial is practised, that as soon

as the decomposition of the body has begun, the gases which are extricated will find egress and mix with the atmosphere, rendering it more or less offensive and impure, and consequently a medium of spreading contagious diseases, that may be introduced within the sphere to which such impure air may extend." (Hosack. Med. Pol. p. 42.) It is true, vaults may under certain circumstances be admitted to be innoxious; but as Dr. Pascalis very well observes, (p. 113) they cannot be depended on always—decay, accidents or neglect, are continually at hand to undermine their supposed safety. It is to be hoped therefore, that the very learned and authoritative professor whose opinion we have mentioned above, will ere long acknowledge the danger of vaults, and extend his proscription to them as well as to graves, and that his testimony will have sufficient weight and influence on the minds of the proper authorities, to cause their entire prohibition, and to promote the establishment of appropriate places out of the city for the disposition of the dead.

The next subject to which Dr. Pascalis calls the attention of his readers is the *Topography of the city of New York in relation to public health.* As we cannot follow him through the description of the city—of the condition of the streets, slips and wharves—of the state of the water, &c. &c. we shall take the liberty to refer our readers, or at least those among them, who are not themselves acquainted with New York, to the book before us, and content ourselves with a very few words on the condition of its burial grounds. These Dr. Pascalis carefully enumerates—also noticing their extent, and the number of vaults they contain. He very properly directs attention more particularly to Trinity church yard, situated in Broadway. It is nearly two acres in extent and is filled.

"A few circumstances," he remarks, "particularly deserve notice: one is the elevation of this cemetery above the streets on the west, it being twenty-seven feet at least above the water line; and the other is its level surface, affording no means of escape for water which of course gives rise to springs."

From this enumeration it appears that there is at the

slightest computation ten acres, or five hundred thousand square feet of ground in the city, exclusively appropriated to interments in graves or vaults, without counting the suburbs and the village of Greenwich, where perhaps twenty acres are divided between the different congregations, besides Potters' Field.

"We will take the subject in another point of view, to ascertain whether the space thus employed may endanger the health of the inhabitants. On the authority of observation and experience it has been elsewhere stated in this work, that it takes more than ten years for the entire decay of the human frame in graves, and much longer than that in vaults." "The following is drawn from the yearly bills of mortality at the city inspector's office, for the last eleven years:

1812,	2505.	1818,	3265.
1813,	2339.	1819,	3176.
1814,	1974.	1820,	3515.
1815,	2507.	1821,	3542.
1816,	2739.	1822,	3231.
1817,	3527.		

Allowing for more than one half of 1823, ... 1625.

"We have here then a total of 33,945 dead bodies dispersed and accumulated within an area of three miles, during eleven years and a half; all still under the decomposing operation of nature, and diffusing in the warm season their volatile exhalations in the air we must respire."

Dr. Pascalis next presents us with a very able "Refutation of the objections raised against the prohibition of interment in the city." He remarks, that as far as may be gleaned from the public papers and private discussion, the prohibition has been by some called illegal, and is objected to on three grounds.

"1st. Because the right of interment obtained by purchase or inheritance is, by the statute, as unalienable a property as any other. 2d. That medical men deny the supposed pernicious effects of places of interment. 3d. That it would be difficult, expensive and inexpedient to transport the dead to a distance."

Against the first of these objections Dr. P. adduces many judicious remarks, which we would recommend to the attention of the profession and of the public in general, but which from the already great length of this article, we are

not able to notice particularly. Suffice it therefore to say, that he has proved at least to our satisfaction, that the municipal authorities *have* the right to remove, for the sake of the public health, the right of burying in vaults, on the same principle that they may take possession of a house or lot for the public use or convenience. The second objection is founded on a medical opinion authenticated by six signatures, and presented to the board of the Corporation of New York, purporting that cemeteries and vaults for interment within the city are not dangerous. Having in the course of our discussion presented many facts that prove the erroneousness of this opinion, we pass this objection by without further comment. In respect to the difficulty, expense and inexpedience of transporting the dead to a distance, constituting the subject of the third objection, Dr. Pascalis remarks that they are not insurmountable, though he confesses that the fatigue and consumption of time, with the expense attendant on following the remains to the grave, are inconveniences, and must be felt keenly.

In the fifteenth and last chapter, Dr. Pascalis proposes the plan of a general cemetery, or polyandrium, which would be, we believe, in many respects worthy of adoption, or at least merits the serious attention of the public authorities.

Art. XIII. *The Institutes and Practice of Surgery, being the outlines of a Course of Lectures.* By William Gibson, M. D. Professor of Surgery in the University of Pennsylvania, Surgeon, and Clinical Lecturer to the Alms House, Infirmary, &c. Vol. I. 8vo. pp. 469.

THIS is a work of very interesting character, and will command the attention and respect of the profession, for many reasons. As being true to its title, an exhibition of the principles and practice of surgery—as comprizing a large body of most valuable information, and presenting the

rich experience of a man who has long been, and is still, ardently devoted to his profession—its claims on our regard are of the strongest kind.

A majority of the books published on surgery, are either distinct treatises on particular affections, or *systems*, suited rather for those who are already tolerably well acquainted with the science, than adapted to the capacity of such as are beginning their studies. Hence the systematic works are of comparatively little service to the beginner, because it is too generally taken for granted, that he is possessed of an acquaintance with the fundamental principles of the art. The facts and deductions are laid before him, when he is unprepared to investigate or decide, and the most he can do under ordinary circumstances, is to acquire some ideas of routine, without having learned to perceive the great general truths which are to govern his subsequent advances.

It is one of the merits of Professor Gibson's work, that a different mode of instruction is pursued. He commences by a concisely perspicuous detail of the principles, and deduces therefrom, the outlines of a system of practice—he describes with graphic skill the symptoms and consequences of surgical diseases, and presents the facts which have fallen under his own observation, in the way best suited to make them serviceable, after the student is prepared to examine them for himself.

The style in which the work is written is of a singularly excellent character. There is no attempt made to fire the imagination—no ambitious effort to substitute rhetoric for reason: he addresses himself to the understanding alone. The eloquence he displays is the eloquence of perspicuity, energy and truth, in the detail of useful facts, or in relating important conclusions. His sentences are brief, terse, and to the point. Superfluous words are carefully avoided—all has been done to illustrate the *sense*, nothing has been introduced merely for the sake of *sound*.

Nor is it only the beginner who may read this work with advantage. Almost every man who is engaged in the multifarious practice which we are obliged to attend to in

this country, has felt the want of a book, in which on an emergency, he can find a brief and decided mode of practice, or operation recommended. This is the more desirable, as the number of surgical cases occurring in country practice, are not sufficient to give a practitioner that habit of thinking and acting, which enables him to be always ready to operate—yet cases do occasionally demand his attention, and he is seldom able to call on a more experienced friend for his advice. There are certainly many works from which a practitioner may derive all necessary aid—but it is a matter of very great importance to him, whether the directions which are to govern his conduct, are to be selected from an investigation of several parallel cases, or are presented to his mind in a condensed and effective form, by one who has studied, experienced and practised a great deal. In addition, he will find appended to each section, a reference to the best sources of information on each topic, and the author assures us, that he has in no instance, referred to any authority which he has not read with careful attention. The value of such references, will be best appreciated by such persons as have had occasion to observe, what a multitude of books are often to be examined, and how much time is spent in discovering, that they were not worth the labour of the search.

Dr. Gibson's work is the outline of a course of lectures which he delivers in the University of Pennsylvania—perhaps one of the fullest and most satisfactory courses of surgery, delivered in the world. We have on a former occasion, published a sketch of his method, the peculiarity of which, is his *imitations* of various surgical diseases on the dead body, and elucidation of others by beautiful enlarged drawings, casts and models. He constantly endeavours to instruct his class, by enabling them to *see* and understand the different modes of operating, and the apparatus to be employed—as he has felt how much easier it is to make a strong impression by laying things before the eye, than merely by addressing descriptions to the ear. The superiority of this method, is well attested by the pleasure the stu-

dent experiences in attending such lectures, and his more perfect retention of the principles and practice taught.

The typographical execution of the first volume, is of a very superior character. The plates, which are numerous, and highly finished, are from the hands of the most eminent artists—and the representations of diseases, faithfully copied from nature. The subjects contained in the first volume, are very numerous and interesting, commencing with inflammation.

We shall expect the second volume of this work, which will appear during the next summer, with considerable interest, and have no doubt, that it will maintain the character acquired by the first. We have restrained from offering any particulars of the work at present, because we shall have an opportunity of recurring to it, when the whole is concluded, and then we expect to present our readers with an analysis of the most interesting details.

ART. XIV. ***Eulogium commemorative of Jason O'B. Lawrance, M. D.*** Delivered at the request of his Class, by SAMUEL JACKSON, M. D.

DEATH, though presented to our contemplation in a thousand shapes—though every moment of created time has proclaimed, loud as the trumpet's tongue, that it is the inevitable destiny of mortals—yet is it irreconcilable to our thoughts—nor can we regard it with the complacency of a natural event. We revolt against it, as though a violence our nature suffers, and an occurrence not connected with our condition.

The Christian, confiding in the truths of the sublimest of faiths, beholds in one unclouded prospect, Eternity expanded to his view and given to his hopes. No sceptic doubt shakes for an instant the firmness of his belief—and as he feels, so his religion assures him that he is the heir of eternal life. He has for his surety the word of his God. The sun may be hidden in endless night—the planets reel from their spheres—the stars desert the firmament—the vast universe dissolve and be no more—but that word endureth forever. Neither is he who wanders in the darkness of Pagan superstition or of Infidel blindness, devoid of this animating belief. Immortal longings stir within his bosom. A future existence is darkly shadowed out to his mind, in the faint gleamings of natural reason ; but it is embodied in all the grossness of mundane conceptions. In him, this belief is rather a feeling from instinct, than the conviction of a revealed or demonstrated truth. So natural is this sentiment to the human mind—so inherent in its constitution, that no period of society has existed when it was unknown. It shone brightly on the morning of our race—it was never completely lost in the darkest era of our degeneracy—now culminating in this epoch of mental supremacy and religious regeneration, it spreads the splendour of the cloudless and

mid-day heaven on the hopes of man. It has been the confiding expectation of the ignorant and uncivilized—the conviction of the philosopher—the trust and consolation of the Christian.

Humanity, elevated by this Heaven-inspired sentiment to the high destinies of immortal natures, in its conceptions is ennobled and dignified, in its affections is purified and refined. Some trace of it is manifested in every action, and imparts a grace and a beauty to feelings that originate in our weakness. It fills us with "thoughts beyond the limits of this frame."

So intense is the abhorrence of annihilation implanted in the heart—so earnest its aspirations of existence—that ingenuity and art have been exhausted on this frail tenement of perishable matters to preserve it from natural decay. Man has vainly striven to reverse the immutable decree of corruption that has been pronounced on his earthly tabernacle—by which it is doomed to the dust. "Dust thou art, and to dust thou shalt return," is the irrevocable sentence from which no mortal has found immunity. Involuntarily we shudder at the grave that closes on our earthly career, as though its gloom was irradiated by no light bursting through the portals of another world; and, despairing of a future being, we labour to perfect a perpetuity to our remembrance here, and to spread a lasting lustre around our names.

That we thus rebel against our fate and war with our destiny, is from nature. Science and religion in vain inculcate that the exanimate form is but as the clod of the valley, and that the remembrance of this world is the emptiest of vanities. We cannot at once forget that the lifeless corse was the centre and object of our love, of our hopes and fears, our pains and pleasures, our tender solicitudes and affectionate anxieties. We cannot at once divorce it from our hearts, and dismiss it to forgetfulness, tearless and without emotion. The memory we cherish, and the virtues we revere, we endeavour to rescue from the oblivious wave that sweeps o'er the myriads of the human race. We would

build up for them a space in the vast ocean of time, whereon we would erect them, as tokens and memorials to excite admiration, to awaken emulation, to instil a lesson to the countless multitudes unceasingly borne on its swiftly flowing currents into the bosom of eternity. The patriot and hero we consecrate in the registers of history—the moralist and sage are perpetuated on the page of philosophy—love and friendship enshrine their objects in the rich emblems of poesy—the proud mausoleum records the honours of the wealthy and the great—and the frail and humble memorial relates "the simple annals of the poor."

Feelings having their origin in this principle, depute to me the office I now perform. You have been deprived of your preceptor. In the midst of his exertions to advance your improvement—while listening to his instructions, and partaking the treasures of his acquirements, he was suddenly torn from you—his tongue hushed in eternal silence, and the lamp of his knowledge extinguished forever. Your experience of the value of his lessons taught you to appreciate their loss. It was not a calculation founded in selfish views that governed your conduct. In your instructor you lost your friend—I may say the companion of your studies and scientific pursuits. For such was the ardour with which Dr. Lawrance engaged in his duties, that he seemed to evince an interest not less deep for his own improvement, than for the information of his pupils. Such was the earnestness of his manner, that subjects the most familiar were handled with all the warmth of novelty—and to his class, while he poured out the abundance of his information, he appeared the most diligent and eager to learn. Delighted with science—never satisfied with the extent or sufficiency of his attainments, he never lost the ardour or intermitted the application of a student. He appeared to you less like one who had already reached the heights that crown the steep ascent of knowledge, and who pointed out to you the paths by which they are attained, than as a fellow traveller whose experience guided your way, and whose hand aided you over its difficulties. In him you found, happily blended, all the

knowledge of the teacher, with the frank and familiar deportment of an associate. The deep regrets and sorrow with which you were penetrated, by the premature death of Dr. Lawrance, cannot surprize, when the relation that became established between him and his class is known. You regret the loss of your instructor, to whom you looked for improvement—you mourn the friend whose manners and whose kindnesses endeared him to you. That you should wish to offer every honour to his memory, is due to his services and your feelings of obligation—is equally just to his deserts, and creditable to the amiable and generous principles that govern the conduct of youths engaged in the refined and elevated pursuits of science. On the spot that covers his remains, in monumental marble you will record the testimony of your gratitude and respect: You have invited me to give an utterance to your sorrow—to express your grief—to portray his virtues—to declare his claims to distinction and regard, too early extinguished by an inexorable destiny, and which, displayed by the opportunities of time, would have received a general accordance.

Alas! gentlemen, I fear I shall not do him justice, or execute your intentions. My voice, like the faint cry of the infant, heard for an instant on the passing wind and then is stilled, can give but a momentary extension to his fame. Conscious of my inadequacy to equal your expectations, I felt a diffidence of this office that would have induced me to decline it, but I knew not how to refuse your request, or to deny myself the performance of this sad yet pleasing duty to my friend. But I have the consolation to reflect, that his eulogy will not suffer from my deficiencies. It does not require the pomp of language, the flowers of composition, and the graces of a studied rhetoric, to give it effect. It is already written in your hearts. I mention the name of Lawrance, and I prompt, in the awakened recollections it excites, an eloquence of thought no laboured effort could produce. You behold him such as he was—a child of nature, unsophisticated by the practices of the world—with a bosom that knew no guile, and a mind untainted by sus-

picions—with a temper that asperities never ruffled, and which was reflected in a deportment uniformly cheerful, affable and kind. An enthusiast in his profession, you admired a zeal that no difficulties could abate, and a perseverance that no obstacles could overcome. What to others was toil and labour, constituted with him amusement and pleasure—what they performed as the drudgery of a necessary duty, was prosecuted by him with all the ardour of a passion. In the winter session of this school, not an instant did you observe him to flag under the burden of engagements, that few could undertake—that would have oppressed most. On your daily entrance into these walls you discovered him at his employments—when you retired for reflection you left him in his occupations—you were surprised to find him still engaged on your return. The commencing morning witnessed his labours—and the closing night brought not their desistance. The perception of physical wants seemed extinguished by the intensity of application—and the exertions of the body to be maintained by the excitement of the mind. Such, gentlemen, did you know your preceptor—as such is he indelibly imprinted on your remembrance—as such will you represent him to the companions of your present studies, who had heard at a distance the rising sound of his fame, and had anticipated to have profited under his directions. His virtues and his talents—the originality yet amiableness of his character—his devotion to his profession—his zeal to acquire knowledge, and the rigid adherence to truth that characterised his observations and inductions, and without which genius and learning are of no avail—you will picture with an energy and eloquence, flowing from a lively admiration of those qualities, and the deep regrets you have experienced by their premature extinction. Imbibing your feelings and convictions, while they become sensible to their own loss, they will delight to be associated with you in the commemorative tribute of respect you design to the honour of his memory.

It is not by exaggerated praise that the memory of Dr.

Lawrance is to be celebrated. Incapable of flattery himself, and despising it when alive, it would be injurious and insulting to offer it to him when dead. The highest eulogy he could receive would be a faithful portraiture of his life —his sentiments—his principles. I shall, therefore, on this occasion, confine myself, in the first place, to a concise biography of Dr. Lawrance—and, in the next, attempt to represent the peculiar features of his character, in his capacity as a physician and as a man.

Dr. Lawrance entered on his mortal existence in the city of New Orleans, in the year 1791, to which place his father had migrated from the state of New Jersey, and where he had entered into the connubial state with a lady of Irish descent. He was early deprived of his paternal protector, and the guidance of his youth devolved on his maternal grandmother, who resided near Baton Rouge, on the Mississippi. It was on the banks of this noblest of rivers —in the pursuit of game that frequents the deep umbrageous forests that line its shores—in the enjoyment of rural occupations—remote from the vitiating and contagious examples of a crowded population—that young Lawrance imbibed those principles of sobriety and temperate deportment— those habits of activity and perseverance, of assiduity and application—that fondness for the study of nature—which, developed and strengthened in a future time, became the conspicuous traits of his character.

To this period of his life Dr. Lawrance always reverted with sentiments of delight, as embracing the happiest years of existence. It is then that the warmth of generosity has not been repressed by the chill of ingratitude—and the bosom of youth, unruffled by the boisterousness of the passions, reflects the world in images brightened and chastened on the calm surface of its own innocence and purity.

From these scenes he was transferred to attend for a period on the schools of New Orleans, and subsequently, in his fifteenth year, to the Lower Dublin Academy, a very respectable institution in the vicinity of this city, under the direction of Mr. Chapman. In this seminary he remained three

years, during which he cultivated with diligence the elementary classical literature, that constitutes the usual routine of education in our country. His application to his studies—the mildness of his demeanour, and correctness of his conduct—gained him the approbation and esteem of his preceptors—and on the termination of his course of education, he was honoured by the testimonials of the Principal for his proficiency, and the correctness of his conduct. On retiring from Lower Dublin Academy, he returned to New Orleans, where, his mother having formed a matrimonial engagement with the late Dr. Flood of that city, Lawrance entered on the study of medicine, under the direction of his stepfather. Under the guidance of this gentleman, Lawrance pursued his studies for three years, profiting by his instructions and the numerous opportunities presented by his extensive practice, to acquire a knowledge of the acute and violent forms of disease common to that climate.

In the autumn of 1812, Dr. Lawrance quitted New Orleans to repair to this city, that he might avail himself of the advantages it presents to the medical student, and terminate his studies by crowning them with the honours of her widely and justly celebrated school. Immediately on his arrival, he selected Dr. Physick as the director of his course of reading and instructor in his profession, and whose pupil he remained for three years, until the time of his graduation. In the close intercourse that was maintained by the relative situation of preceptor and pupil, Dr. Lawrance imbibed those sentiments of profound respect and admiration for that truly eminent man, which he preserved during his life. At the same time, he had the happiness to secure, and it is an evidence of no common merit, the entire confidence, the esteem, and friendship of his preceptor, which no subsequent circumstances in the slightest degree tended to diminish. When the close of his preparatory professional studies terminated this connexion, he was honoured by Dr. Physick with an attestation of his singular diligence in acquiring a knowledge of his profession, and

a warm commendation of the peculiar amiability, and the uniform propriety of his conduct.

Whilst an attendant on the instruction of this University, Lawrance became distinguished for the intensity of his application, especially in the cultivation of anatomy—and he is still remembered for the unexampled ardour he manifested in his favourite pursuit. Eager to seize on every opportunity that could advance his improvement, and fit him for the important and responsible duties of relieving the sufferings incident to human nature, he embraced the occasion of a vacancy in the Pennsylvania Hospital, to offer his services to the managers as resident physician. His tender was accepted—and in the wards of that noble institution, dedicated to the most exalted of charities, and under the direction of distinguished medical and surgical advisers, he acquired an extensive experience of the multitudinous forms in which disease and death invade and ravage the frame of man—and the means by which they are to be resisted and repelled. On the expiration of his voluntary services, the managers directed their secretary to communicate to Dr. Lawrance, the expression of their perfect satisfaction at the manner with which they had been performed, and of their "approbation and esteem."

In March 1815, the degree of Doctor of Medicine was conferred on Lawrance by the Professors of the University of Pennsylvania. Few have received this honour with equal preparation to justify its title and to maintain its dignity. He could have obtained, at an earlier period, his diploma, but he was less ambitious to procure than to merit it—to acquire a mere appellation, than to deserve a real distinction—by becoming master of all the groundwork of his profession—by the attainment and combination of the skill and science that can alone constitute the philosophical physician, and vindicate the attempt to regulate, govern and control, in health and disease, the delicate and complicated movements of the animated machine. Having thus qualified himself, by the devotion of six years of assiduous application to his studies—by a diligent resort to the lessons of those

luminaries of medical science, in whom are concentrated, and by whom are reflected, the knowledge of the past, combined with the experience of the present times—by an attentive observation of nature, in watching diseases at the bed-side, in our extensive infirmaries, and by correcting and determining the true character of symptoms from the organic alterations, made known by autopsial examinations—Lawrance felt himself prepared to enter on the duties of the high office, to which he had dedicated his life. With this intention he returned to his native city, and entered into practice. His success was flattering. Had the views of a sordid interest swayed his actions, they could have been gratified to their utmost extent. But he felt that he was removed from the soil congenial to his genius—and the acquisition of wealth could not satisfy the cravings of intellectual appetition. Lawrance was emphatically a child of science. In the busy commercial emporium of the vast agricultural regions of the West, and in its fluctuating population, so many of whom are congregated by the ties of a transient interest, it is not to be expected that Letters, Arts, and Science should flourish with vigour. Lawrance found himself alone, as a stranger in a strange land—few to participate in his feelings, and none to join in his pursuits. He looked back with regret to those associations to which he had been united in this city, and in which the toils of professional engagements are relieved by the recreations of science. He lamented his separation from the companions of his studies, and his competitors in a generous and honourable rivalry, who had stimulated his zeal and encouraged his exertions. He felt as an affliction the deprivation of those opportunities he had here freely enjoyed, to indulge in those investigations to which he was led by the natural bias of his genius—and which, a conscious impression foretold him, would lead to the goal in the career of professional distinction. He thirsted for an honourable fame—and the only prospect offered to his hopes was the attainment of riches. To a vulgar ambition this had been sufficient: but that of Lawrance was of a nobler strain, and not to be appeased by the gratifications of ava-

rice. After three years residence in New Orleans, he yielded to his inclinations, I may say to his propensities. Unseduced by the prospects of gain, and a rapidly increasing practice—unshaken by the remonstrances of friends—he abandoned the city of his birth, to place himself in the midst of the busy throng that crowd, in this metropolis, the avenues of medical science and fill all its departments.

In this step we have displayed the character of Dr. Lawrance. A brilliant fortune would have rewarded his perseverance in New Orleans—but it offered no field for professional eminence, beyond the reputation of a successful practitioner. In this city the emoluments of practice are moderate—are scarcely an adequate recompense for the toil and slavery it demands, even when most successful:—but the profession is lofty in character—rich in honours—and fruitful of distinctions. No other city of our continent enjoys more facilities for improvement, or equal means and inducements for new researches—new developments—for ardent devotion to the advancement of our science: while the numerous sources and objects that excite emulative industry, and give employment to mental activity, call forth dormant talent, and rescue merit from obscurity. Lawrance did not hesitate: he forsook present competence, and an assured wealth, unattended with fame, to encounter certain difficulties—to attempt a doubtful fortune in the midst of active competition, but on a field which gave full scope to his energies, and offered no barriers to the exercise of an honourable and legitimate ambition.

During his residence in New Orleans, the ravages of that most formidable and destructive of febrile affections—the endemic of the West Indies, and periodical scourge of this continent—enabled Lawrance to investigate its character by numerous dissections. The danger to which he was exposed by repeated and prolonged examinations of bodies, festering with corruption in that ardent climate, could not alarm his apprehensions or diminish his perseverance. By day or by night, whenever an opportunity offered, he hastened to interrogate nature herself, by a minute inquiry into

the organic lesions, what were the peculiar seat and character of the disease. The observations he thus collected were carefully recorded—and the most copious autopsial examinations of yellow fever that exist, are contained in the manuscripts of Dr. Lawrance. This valuable collection, it was apprehended, would have been entirely lost, by his premature death. Happily, we are promised by his warm friend and consociate in similar pursuits, Dr. Richard Harlan, a posthumous work, arranged from the voluminous manuscripts of Dr. Lawrance, that will present to the profession the fruits of his extensive inquiries in pathological anatomy.

No sooner had Lawrance established his residence in this city, than he entered on the course to which he was borne by the strong bent of his inclination, and commenced a career that only required the maturity of time to have ensured to him fame and distinction. Anatomy, natural and morbid, and experimental physiology, were selected as the objects on which he concentrated his attention.

The knife of the anatomist has too long been industriously applied with skill and intelligence: the scalpel has laid bare every fibre, and demonstrated every tissue too often, to leave a hope that any important discovery in the human structure remains to be disclosed. It was not a fallacious expectation of this nature, that led Lawrance to attach himself with so much devotedness to acquire a perfect acquaintance with human anatomy. He regarded it, justly, as the only basis that would enable him to form a scheme of investigations—illustrating pathology by dissections, and physiology by experiments—entitled to confidence, or that could be productive of profitable instruction. Without the most absolute familiarity with the natural structure, it is impossible to appreciate, with judgment, the alterations that are induced by disease—to discriminate recent from ancient changes—and the various species of morbid degenerations to which it is subject: without a minute knowledge of the organic structure, it would be futile to attempt to remove the veil that conceals the mysteries of

functional operations, by experimental investigations—they would of necessity be defective, from want of skill in conduct—of consistency in arrangement—and conclusiveness in induction. Convinced of the correctness of these principles, Lawrance never intermitted, though he had arrived at great proficiency, his anatomical studies. At the same time he regularly attended the great Infirmary of this city—watching the progress of cases to their conclusion, and determining the morbid changes of structure that characterised the disease.

Pathological anatomy has been singularly neglected in the United States. Except some limited essays, amongst which the dissections of Dr. Physick in the yellow fever of 1798, are the most distinguished, I know of no systematic work that any American physician has contributed to the improvement of pathology by anatomical illustration. Yet it is indubitable, that without the assistance of morbid anatomy, organic diseases cannot be understood by the practitioner, or their progress arrested by a systematic treatment. Though of less immediate utility, in diseases resulting from lesions of the vital powers, still their nature and character are often exemplified by the organic alterations they induce, and the method of cure rendered more determinate and consistent. This deficiency, with which the profession of this country may with great justice be reproached, Lawrance would have amply supplied, had a greater prolongation been granted to his existence. When we regard the poverty of our science in this department, the preparation of Lawrance for its execution, and the advancement he had already made in this important work, we become more fully sensible to the loss that has been sustained in his death.

The pathology of the cerebral diseases is involved in confusion and uncertainty. This organ engaged the attention of Lawrance. He studied its structure with a patience truly exemplary. He had become completely a convert to the opinions of Gall and Spurzheim, and their method of dissection, which he generally adopted in his examination: and the correctness of which, with their peculiar doctrines

of its organization, he satisfactorily demonstrated. It was his object to render himself, by repeated dissections of this organ, familiar with the natural appearance and condition of every portion, that he might be enabled to detect the slightest changes resulting from disease, and to profit by the opportunities that should offer, to seize on facts calculated to enlighten the darkness of its pathology. Often has his unwearied attention excited admiration—as, day after day, he repeated the minute dissection of this complicated structure. His introduction to the practice of the Alms House, where the various forms of cerebral disease—mania, apoplexy, convulsions, palsy, and epilepsy—annually swell the hecatombs of the dead—had placed him in a position to have rendered productive of useful results, this devotion of time and labour, and which, there is every reason to believe, would have contributed richly to the stock of our information.

Soon after he had taken up his residence in this city, Dr. Lawrance began to attract attention, by his active and unremitting exertions in the pursuit of those objects which engaged the energies of his mind. In the spring of the succeeding year (1820) a fever, of peculiar character, made its appearance amongst our black population, who were, with few exceptions, almost exclusively its victims. At no prior time, I believe, had this disease been noticed. Certain it is, that never before had it assumed the character of an epidemic, and extended its ravages so widely. It is remarkable, that this febrile affection, which has continued to manifest itself in that portion of the community, each succeeding year, first exploded in the month of May, a period when our white population is, with few exceptions, uniformly exempted from the attacks of continued fever, and that it declined with the advancing heats of summer, and had reached its termination in September—the season when the fevers of the whites are most prevalent. This circumstance continues to be remarked as peculiar to that disease. The vague and indefinite name of Typhus was, and is still, applied to this febrile affection—leading to a misconception of

its true character, and often to a very improper mode of treatment. In the investigations of the nature and seat of this disease, as indicated by post mortem examinations, Lawrance was indefatigable. His examinations were frequent—were daily repeated—and we are indebted to his researches, and those of his friend and associate, Dr. Harlan, for determining its character to be a gastro-entiric fever, of irritative and inflammatory character. During the same year the reappearance of yellow fever, which we had been led to flatter ourselves, from its long absence, had taken its departure, and would not again affright the city with its dreaded presence—enabled Lawrance to resume his researches on the morbid lesions it induces, and contrast its character, in our more Northern latitude, with what he had observed in the Southern section of the country. The deep interest he felt in these inquiries, caused him to disregard the dangers resulting from reiterated and prolonged dissections, in the sultry heats of summer. They were daily renewed, particularly amongst the victims of the epidemic of the blacks —and were rendered more perilous, from attempts to ascertain the minute alterations of the important structures, especially that of the brain. He did not escape the hazards to which he thus exposed himself:—after the examinations of several subjects, continued for some hours, he was seized with the usual febrile premonitions, which ushered in a severe attack of fever. For a few days his life was considered in imminent danger, and his recovery despaired of. A vigorous and unimpaired constitution, and the skilful attention of Doctors Physick and Chapman, enabled him to triumph over his disease.

In the fall of 1820, Dr. Gibson, the Professor of Surgery, engaged the services of Lawrance as an assistant in his department—which situation he continued to enjoy to the period of his decease. In this capacity the Professor of Surgery has acknowledged his usefulness.

In the spring of the same year, Lawrance commenced a course of private instruction on anatomy and surgery, which was delivered daily, with the exception of the month

of August, until the month of November. The same course was repeated each succeeding year, to an increasing class, and was suddenly terminated the last summer by his lamented death. The time occupied by this course is an evidence of the fulness and completeness of the instruction he spread before his pupils: nothing was omitted that should be communicated—nothing left obscure and to be conjectured—but every point was amply and fully dilated. A friend who was in constant communion with Lawrance, has declared that he delivered "one of the fullest courses of lectures ever given in this city."

In commencing this arduous undertaking, Lawrance did not, as young men are prone to do in the exuberance of fancy and youthful ambition, mistake his powers and overrate his strength. He rose from the task with a reputation tested and confirmed by trial. His class awarded to him the suffrage of their approbation—and with the students, who are the best judges of those qualities that become an instructor, Lawrance was an esteemed and favourite teacher. Disciplined by experience and familiarized by habit, the manner and delivery of Lawrance rapidly improved—and he bid fair to become in time a lecturer as interesting as he was instructive, and to justify the expectations of the most auspicious fortunes.

The numerous avocations in which he was engaged, did not divert the mind of Lawrance from the experimental inquiries into the vital functions, in which he early contemplated to engage. In 1821, an association was formed by a number of the physicians of this city, for the improvement of the science of medicine. The experiments of the French physiologist, Magendie, on venous absorption, had then lately been made known. It was an interesting and important object to verify his conclusions, novel in modern physiology, and which are probably destined to have an extensive influence on our speculations in pathology and therapeutics. A committee, consisting of Doctors Lawrance, Coates and Harlan, was appointed, to repeat those experiments, and test their accuracy. The want of funds, that so frequently embarrass the experimental investigations of the

young American practitioner, was freely supplied by Professor Chapman. With that liberality he has manifested on so many occasions, and which constitutes a brilliant and distinguishing feature of his character—he placed a hundred dollars in the hands of the committee, with instructions to call on him for any additional sums that might be required. This committee did not confine themselves to a mere repetition of the experiments originally executed by the French physiologist, but after ascertaining their accuracy, varied them—devised new—corrected some, and strengthened most of his inductions by fresh observations. It would be unjust to the other gentlemen of the committee, to attribute the successful result of their undertaking to Dr. Lawrance, yet they will cheerfully acknowledge the important aid they derived from his skill—the value of his suggestions in framing their experiments, and the caution with which he guarded against error from a too precipitate adoption of results. The labours of this committee, and the complete character of their investigation, may be conceived from the extent of their experiments. Not less than ninety experiments were performed on living animals, before they felt authorised to adopt the doctrine of venous absorption.

Aware of the many instances of deceptious inferences into which philosophers have been led by imperfect inquiries, and the baneful influence they have exerted on our science, Lawrance never desisted from an inquiry until every possible source of error had been resolved. Notwithstanding the minute scrutiny of the committee, it appeared to Lawrance that some points had been neglected, by which the colouring matters might be supposed to have found their way into the blood, independent of the veins, in some of the experiments in which that result was indicated. Although satisfied in his own mind, he considered it a duty not to rest contented until every shadow of doubt had been dissipated, and every objection answered. He accordingly commenced a new series of experiments, in conjunction with Dr. Coates, in order definitively to determine this question. The former course of experiments was repeated, diversified,

and varied on more than one hundred animals, providing against every contingent result, and guarding against every possible fallacy.

The doctrine of venous absorption, one of the earliest of medical theories, though abandoned on the discovery of the absorbent vessels and their office, revived by Dr. Magendie, and in part established by his experiments, was thus positively confirmed by Dr. Lawrance and his associates—and by their researches is recorded amongst the most clearly ascertained facts of physiology.

The procedure of this committee, I would recommend as a model for investigations of this nature. In no instance was a single experiment, ever liable to mistake, relied on: repeated and varied, no point where error could harbour was left unexplored—and the conclusions, deduced from numerous confirmatory and concurring results, removed from the debatable questions that agitate and perplex our speculations, have become fixed and demonstrated truths. It is alone by this Baconian method of philosophizing, that our science can be rescued from that state of interminable controversy in which it has been involved for ages, resulting from unfaithful observation, imperfect experiments, and an absolute looseness of deduction.

The determination of this question left the leisure moments of Lawrance disengaged. His active mind immediately seized on a new subject of investigation. No absorbent vessels have yet been detected in the brain. Subject as this organ is to effusions of blood and of serum, constituting the most formidable diseases which enter into the domain of practical medicine—its powers of absorption is a question of pathological, therapeutic, and physiological interest, that has not yet been solved. In this inquiry Lawrance engaged with his friend Dr. Coates. A few experiments alone were performed—establishing, however, the fact of absorption. The consecutive researches to decide the nature of the vessels, and mode by which this function is accomplished, were interrupted by the death of Dr. Lawrance—and our positive information on this very important

subject is postponed, until some one with equal zeal, industry and attainments, will undertake the task.

In the winter of 1823, in addition to his employment as assistant to the Professor of Surgery, Dr. Lawrance engaged with Dr. Horner, the adjunct Professor of Anatomy, in superintending the anatomical demonstrations of the dissecting classes, which were that year more than usually numerous, and in a course of anatomical surgery. Multiplied as were the duties of these various undertakings, they were satisfactorily performed—and with the increase of his engagements was enhanced the reputation of Dr. Lawrance, as a demonstrator and teacher. His labours during that winter, though severe, were sustained with an unwavering perseverance. Six hours was the period he allotted for rest—and very frequently, such was the activity and excitement of his mind, that it did not extend beyond four.

The close attention of Lawrance to these objects, did not lead him to a neglect of his professional duties. His practice, it is true, was not extensive. A stranger, and a resident only within a few years, it could not be expected that he should have rivalled the veterans of the profession, or even have overtaken its younger members—surrounded and sustained in their native city, by connexions and friends. Yet, within the last two years it increased so rapidly, both in extent and respectability, that his singular industry and economy of time alone, could have permitted him to have continued the accessory investigations that have been noticed. His zeal carried him beyond the limits that prudence would have dictated, and led him into exertions dangerous to his health and safety. He had been elected, by the Managers of the Alms House, one of the surgeons to that institution. The opportunities that it presents, for the illustration of pathology by dissection, Lawrance was anxious to improve. His associate in the medical department, and etertaining a similarity of views, we became united in this pursuit. For several weeks previous to his decease, our autopsial researches were daily renewed, and in which Lawrance often subjected himself to exhaustion and fatigue, by

the avidity with which he ravelled the minute anatomy of the brain.

The epidemic fever, which for the last three years has ravaged the vicinity and suburbs of our city, had again commenced its depredations—and Lawrance was frequently called on to afford the assistance of his art. His visits were made on foot, in the heat of the mid-day sun, and the damp dews of the night. He was thus at once occupied with his official duties at the Alms House, with a series of observations on morbid anatomy, conducted in the same institution, in conjunction with myself—with a summer course of anatomy and surgery, delivered with a fidelity and ability, of which your regrets for his loss is an impartial and unequivocal testimony—in a series of experiments on cerebral absorption, in association with Dr. Coates—and in a practice extensive and unusually laborious. He may be considered censurable for crowding his time, at an unhealthy season, with so many employments of importance—but the incessant occupation of Lawrance, in the University during the winter, precluded the researches and experimental inquiries he was eager to achieve—and his stirring ambition to be useful, would not permit him to postpone their execution to periods of remote and uncertain disengagements.

Debilitated by want of rest and over exertion, his system, imbued with putrid miasms, from frequently repeated dissections, was prone to disease. Assisting Dr. Physick, in the commencement of August, at the performance of an important operation, he exhibited evident symptoms of indisposition, but which were attributed to fatigue. They were the precursors of a mortal fever, that already preyed on his vitals—and which, the same night, displayed itself with frightful and portentous energy. His medical friends, alarmed at his danger, surrounded his bed, and were lavish of their attentions. They had to mourn the impotency of their resources, and the imbecility of their art, when Death, commissioned to destroy, stretches forth his sceptre and demands his victim. Vain are all human means, in this unequal conflict. Like the vessel, gallantly speeding o'er the

booming waves, freighted with the rich productions of every clime, on which the destructive typhoon comes to burst in its fury—whelmed with the surging deep—no skill of the pilot can save—or the brave exertions of her intrepid crew.

Thus perished Lawrance, in the prime of manhood—in the pride of his days—in the period when every auspice shed its happiest influence o'er his path, and brightening prospects cheered him on his way. To the erring judgment of mortals, his career seemed destined to be brilliant with every hope, and its long vista to terminate in a rich expansion of rewards and honours. But where are they? The grave has closed upon them all. Himself—his hopes—his dazzling prospects, and his fame, just bursting like the dawn, are cold, withering, darkening in the tomb—renewing the oft-repeated, still-neglected lesson, of the uncertainty of our condition, and the vanity of mere earthly expectations.

In the life of an individual we have displayed his character. It is unnecessary I should dwell on that of Lawrance, already exhibited, in its principal traits, by the incidents of the preceding biographical relation. He was distinguished less for brilliant than solid endowments. The imaginative faculties were repressed, rather than cultivated—and he slightly prized the embellishments of the intellect, too often incompatible with the maintenance of its vigour.

Medicine is a science of strict observation and rigid induction—a talent and mental operation, that the visions of fancy delude, and the phantasms of the imagination pervert. These faculties, corrupting medical philosophy with their pernicious influence, have ever diverted it from the plain path of demonstration, into the mazy windings of conjecture—have loaded the science with that mass of false facts and false theories, that obstruct its progress and dishonour its character. Quick perception—persevering application—patient attention—unweariable industry—professional zeal—those are the qualities to which medicine is indebted for all its substantial improvements. Those are the qualities,

that, predominating in the character of Dr. Lawrance, admirably qualified him for the painful and difficult investigations, by which truth, in science, is to be elucidated.

The public contributions of Dr. Lawrance to the profession, are inconsiderable. He did not consider himself as having reached that period, when multiplied experience and maturity of judgment, diminish the probability of error—and no feverish anxiety for distinction, goaded him into a hasty promulgation of opinions on subjects, at once obscure and important—which have baffled the reasoning of the wisest, and in which error is pregnant with dangers. His work of preparation was, however, extensive, and the materials he collected voluminous. Upwards of three thousand manuscript pages, consisting of observations on diseases, histories of cases, and necrological examinations, he has left behind—attest the industry and devotion with which he applied himself to the means of the improvement and the advancement of his science.

Towards his profession, Lawrance entertained an enthusiastic attachment. He did not regard it as a business which gratified his individual and social necessities, but as the noblest ministry in which the rational and feeling mind can engage. "Man," says Cicero, "approaches to the Gods in nothing so much as in giving health to man." And one of the most acute of modern philosophers has remarked, that—"Those who would apply themselves to restore others to health, from the sole principles of humanity and benevolence, would be above all the great ones of the earth—they would partake of the Divinity: to preserve, and to restore, is little less than to make." Animated by similar sentiments, Lawrance pursued his profession as a philanthropist. The poor and the rich knew no difference in his attentions—which were governed by the urgency of the disease, not the expectations of reward. His principles of practice belonged to the modern school, based on pathological anatomy and physiology, and which, restoring medicine to its character of a sublime philosophy, distinguishes the philosophical physician from the practitioner of routine.

In private life, Lawrance was peculiarly felicitous. He lived without reproach. I truly believe that censure never breathed upon his name. Governed by principle, a rigid punctuality attended his engagements, and a regulated deportment directed his actions. His mind, intensely occupied on his professional researches, pleasure's syren voice could never charm his ear, or ambition's dreams bewilder his understanding. Ignorant of the arts of policy, he knew but one way to command success, which was to deserve it—and when it lighted on him, none justled from his path—envy was hushed, and it was confessed to be merited. With many warm friends, I know not that he had an enemy.

Grateful as is the theme, why should I dwell on his professional merits, and personal virtues? You knew them well, and this day attests how truly they were appreciated. Though he has been early torn from his friends—from society—from our science—his labours incomplete and his task unperformed—yet let him not have lived in vain. He has left his example, and I do not fear to propose it as a model for imitation in your profession. You are about to enter on a ministry involving the most awful responsibilities. You will ask to be entrusted with the health and the lives of your fellow mortals: the husband to confide to your skill the safety of his spouse—the wife, that of her husband—the parent to commit to you his child—and a helpless family to depend on you for the recovery of a father and protector, whose death would plunge them into wretchedness. An error of judgment—mistaken views—deficient information—negligence in attention—may be fatal, and you, the authors of the heaviest calamity that man can suffer. If you would avoid the deep-felt censures of your own conscience, go, then, and follow in the path of your preceptor: Like him grow pale in the sickly wards of hospitals, watching by the squalid bed-side of disease—pine o'er the midnight lamp recording your own observations, or consulting, in their works, with the experienced living and the illustrious dead—subduing every feeling of abhorrence and disgust, with defiled hands tear from the cold and lifeless

corse the mysterious secrets of disease, that you may know, of each, the causes and the seat. Believe me, gentlemen, that could our departed friend listen to my invocation, and touch my lips with the inspiration of his spirit, such would be the injunction, that, from the grave, he would pour into your ears. Take it then as his last counsel—practice on it: conform your professional conduct to those precepts. It is the highest honour his pupils can offer to the memory of Dr. Lawrance—and you will perform your duty to yourselves—to society—to your God.

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

ANATOMY AND PHYSIOLOGY.

M. L. L. Jacobson *on a Humour of the Eye little known.*—We have had more than one occasion to mention the researches of M Jacobson, of Copenhagen, who thinks he has reason to conclude—that a serosity or watery humour naturally exists between the choroid coat and the retina, and contained in a duplicature of the latter—that a minute portion of the same humour also exists on the corresponding anterior part of the retina—that this humour sometimes accumulates in so great a quantity as to push forwards the retina and the vitreous humour, and to produce the disease, called by Scarpa, *Staphyloma posterior,* by distending the choroid and sclerotic; and that this humour can be displaced by a calculous deposit, which is usually, and erroneously, considered as an ossification or petrefaction of the membranes of the eye and of the vitreous humour, —*Nye Hygeie, Copenhagen.*

Professor Isenflamm *on the Constituents of Tendons.*—The School of Erlangen appears to be rising into respectable notoriety. Professor Isenflamm, one of its ornaments, has instituted some experimental researches into the chemical constituents of tendons, and seems to have established, that they differ little from muscles, with the exception of their being destitute of salts. By carefully macerating and dissecting tendons, he came to the conclusion—That they are formed of cellular membrane, arranged in fibres, accurately parallel with tendinous fibres crossing them, and forming a membrane of animal gluten and of albumen, from which, and the fibrous texture, the satiny appearance of tendon is produced. He decides that nerves enter into their structure, whether they can be traced or not, from their sensibility when diseased—for according to the Professor, no vital property, such as the nervous, can be generated, though it may be developed by a morbid state of the parts.—*Archives de Médecine.*

M. Lisfranc *on the Uvula.*—In an excellent paper on the Anatomy, Physiology, and Pathology of the Uvula, M. Lisfranc

regrets that he could find little precise and scientific information concerning it in systematic works. The dimensions of the uvula are various. He has seen two subjects in which it appeared like a very short tubercle, though otherwise voluminous. In another case he found it of its ordinary length, but as fine as a thread. These circumstances he thinks could have but little influence on the voice. Hagerdorn, in the *Ephèmèrides des Curieux de la Nature*, gives the case of a girl born without a uvula in whom the voice was unaffected and natural. In the same work Vallgnadius mentions a bifid uvula, in a subject who at the same time had hare-lip. Both Roux and Lisfranc himself have observed similar cases. It is wanting in all animals but man—careful dissection proves that the uvula, when very thin, and represented in a tubercle, is wholly composed of mucous membrane and a great number of follicles—and the bifid state, or the absence of the palato-staphylini muscles in its thickness, will cause its descent. The uvula is formed by these muscles, whose supporting fibres, *(les fibres adossées)* seperate as they approach it, and their prolongation is covered by a mucous membrane enclosing a greater quantity of follicles than has been hitherto pointed out. Under this is a close *(serré)* cellular tissue, studded with small glands, whose organization resembles that of the tonsils, and the thalamus of the crypta agglomerata, found under the mucous tissue of the velum pendulum palati in the horse and other animals. The free extremity of the appendix appears to be destitute of muscular fibres. Here M. Lisfranc has repeatedly found three mucous follicles much developed, very distinct, and susceptible of augmentation, to about a third of the usual length of the uvula. M. Boyer has observed upon this point a small transparent tumour produced by the accumulation of serosity. Frequently the appendix is soft, chiefly towards the top, and makes a similar structure to that of the nasal polypi of the mucous membrane. It is needless to say that it may become scirrhous, carcinomatous, and even cartilaginous, and that it is often in a state of procidentia. —*Revue Medicale.*

M. Lisfranc *on the Uvula.*—The remark of Hagendorn opposes the conclusion that the uvula concurs in the formation of the voice—and many patients who have had it removed by operation, show that it has but little influence on the articulation of sounds. Many physicians know that venereal ulcers have destroyed both the uvula and the pillars of the velum, as well as the epiglottis, in distinguished comedians, and that notwithstanding the character *(timbre)* of the voice has lost nothing of its pliancy and freshness. If syphilis alters the voice, says M. Biett, by the destruction of the uvula, it is not so much

the want of the uvula as the deep morbid taint which the disease has given to the rectum and the parts in its vicinity, and which is beyond the reach of cure. Our author believes with Richerand, that the uvula is destined to advertise the pharynx of the arrival of ailment—and that it furnishes from its numerous follicles, mucus to facilitate the passage of the food. M. Lisfranc, besides, thinks he has discovered and proved that the uvula serves to prevent the nasal mucus from falling into the glottis. Its movements in this point of view may be seen by opening the mouth before a mirror, and strongly drawing in the breath. The strongest proof of this, however, is, that when a complete case of procidentia occurs, or when it has been entirely removed, the nasal mucus very readily gets into the glottis during a slow and long drawn inspiration.—*Revue Medicale.*

M. Edwards *on the Carbonic Acid Gas of Respiration.*—Our indefatigable and scientific correspondent, M. Edwards, has communicated to the Academy of Sciences the results of numerous experiments which he has made upon the exhalation of carbonic acid gas during the pulmonary expiration. He proves, by these, contrary to the opinion generally admitted, that the carbonic acid is not formed instantaneously in the lungs by the action of the air inspired, but that this gas is a genuine secretion of the blood, made in the respiratory organs. In his interesting experiments, M. Edwards reduces cold blooded animals to respire hydrogen gas perfectly pure. This process was persevered in during many hours, as in the atmospheric air. The result was, that after this lapse of time, the presence of a quantity of carbonic acid gas became evident, amounting almost to an equal quantity as would have been furnished if the respiration had been conducted in the open air. This result has been frequently suspected among the numerous conjectures to which the subject has given rise—and when the memoir is published with all its details, we hope it will do much to clear up the difficulties connected with respiration and the changes which the blood undergoes in the lungs. M. Edwards had previously discovered, that azote is absorbed into the circulation, and subsequently discharged from it—and that each of these actions is regulated by the constitution, habit, and circumstances of the individual, and by the influences to which he may be subjected, the absorption being to a small extent, whilst the exhalation is considerable, and the reverse.—*Quar. Jour. For. Med. and Surg.*

Foville *and* Pinel-Grandchamps *on the Brain.*—The researches and experiments of M. Flourens and Fodéra, borrowed it seems in a great measure from Roland's work, "Saggio

clinico sulla vera struttura del cervello, e sopra le fonzioni del sistema nervosa," have led to many interesting facts and observations on the functions of the particular parts of the brain. Among these we may mention a case related to the Académie Royale by MM. Foville and Pinel-Grandchamps, of a female whose upper and lower extremities of the left side has been for several years completely paralysed. On examination they found an old extravasation on the right hemisphere of the brain, in the medullary space, outwardly, between the thalami optici and the corpora striata, and equally affecting these two parts. The authors stated this fact to confirm their former opinions on the seat of motion in the two limbs, namely, that the seat of motion in the upper extremity is placed in the corpus striatum. Hence it should seem to follow, they think, that when paralysis occurs at the same time in both the upper and the lower extremity, the corpus striatum and the thalamus opticus will both be found morbid.—*Quar. Jour. of Foreign Med. and Surgery.*

Proximate Principles of the Blood.—MM. Prévost and Dumas of Geneva, allege that they have found the blood to consist of serum, holding in suspension regular insoluble corpuscles, which are composed of a central colourless spheroid, and a sort of membranous sac, of a red colour, surrounding it. And they consider the coagulation of the blood to arise from "the attraction of the red matter round the white globules ceasing with the motion of the fluid, so that the white globules can obey the force, which tends always to unite them." The white globules are of course fibrine—and the red matter seems to them to be a kind of translucent jelly, insoluble in water, easily divided, but not capable of reunion. They have examined the proportion which the white corpuscles and red matter together bear to the rest of the blood, in a great variety of animals—and they find them most abundant in birds, next in the mammalia, especially the carnivorous mammalia—and they are least plentiful in the cold-blooded animals. In man they constitute about 129 parts by weight, per thousand. They are more abundant in arterial than in venous blood—one thousand parts of the arterial blood of the sheep, dog, and cat, contain 10 parts more of these papticles than blood taken from the veins. The serum is identical in both. Their observations are many of them very interesting: the account they give of the coagulation of the blood is evidently nothing but words.—*Ann. De Chim. et de Phys. Mai* 1823.

M. Magendie *on the Functions of the Spinal Marrow.*—In following up the novel experiments of Mr. C. Bell, M. Magendie remarks that, in general, the properties of the spinal mar-

row appear to reside at the surface of the part—this is at least evident as regards sensibility. If the posterior cords, covered even by their vascular membranes, be touched, we observe signs of an acute pain, and what is worthy of remark, very marked contractions in the muscles which recieve their nerves, lower down than the part touched. The contractions only shew themselves on the side of the cord which is irritated.

It would be, doubtless, very desirable to know how sensation and motion are propagated from the marrow to the brain. The anatomical disposition indicates that sensation should be directed more particularly towards the cerebellum and motion towards the brain—but anatomy is not sufficient: it is necessary for physiological and pathological facts to confirm the indication: until the present time, however, neither the one nor the other of these means has established what anatomy seems to shew in so evident a manner. Lesions of the cerebellum do not cause a loss of sensation. Removal of the hemispheres does not necessarily induce a loss of movement; the contrary assertion, announced by M. Rolando, is not exact: this physician appears to me to have suffered himself to be deceived by an accidental circumstance. When we wholly remove the hemispheres, an effusion of blood immediately takes place, and a coagulum is formed which fills the cavity of the cranium, compresses the medulla oblongata, and produces the state of somnolency *(assoupissement)* observed by M. Rolando. But if we prevent the formation of this coagulum, the symptoms are quite different; the animals are in continual agitation; they run or fly with remarkable agility, provided they are not too much weakened by the loss of blood. The animals on which this experiment succeeds the best, are small rabbits, a month or six weeks old, and young jackdaws or magpies, just beginning to feed alone. It is singular to see them run, leap, &c. of their own accord, after the complete ablation of every part of the brain, situated a little before the optic tubercles. But if the section be made immediately before these last eminences, every thing is arrested—the animal falls upon the side, the head is thrown backwards, the paws entirely stiff and directed forwards. I have seen young rabbits remain several hours in this position. In order to put an end to it, it is sufficient to make a section behind the optic tubercles. Immediately the anterior paws lose their stiffness, and most commonly become bent as well as the posterior, and the head is again brought forwards. It seems to me to be evident from these facts, that the optic thalami, the crura cerebri, and the tubercula quadrigemina have functions relative to motion, and these parts should be examined under this new point of view.

The effects of a partial or total removal of the cerebellum

are much more difficult to observe, by reason of the great hemorrhage which always accompanies a wound of that organ, of the effusion which is the inevitable result of it, and of the compression of the spinal marrow. I have not yet been able to assign to each of these effects, the part it takes in the phenomena which occur at the time of wounds or ablation of the cerebellum: it is easy however to prove, that profound lesions of the cerebellum, and total ablation of it, do not cause the loss of sensation. The experiments of Larry, Legalois, &c. have, besides, demonstrated that this quality is inherent in the spinal marrow. It is to be hoped that this difficulty will be soon removed, for several zealous individuals are occupying themselves with researches on this point, and I am myself using all my endeavours to arrive at something satisfactory on this important question.

What I have hitherto remarked most constantly is, that the cerebellum seems necessary for the integrity of the forward motion. Every triflingly severe wound of the cerebellum totally prevents progression, and most commonly developes, on the contrary, a set of movements which belong to the action of retrogression. A duck, from which I removed a great part of the cerebellum, could swim backwards, and made no progressive movement for eight days.—*Journ. de Phisiol.*

Functions of the Spinal Cord.—Not long ago, as our readers are aware, M. Magendie found by experiment, that the anterior bundles of the spinal nerves are destined for motion, and the posterior bundles for sensation. Pursuing his inquiries farther, he appears to have established the same facts with regard to the anterior and posterior portions of the spinal cord itself. If it is touched or pricked on the anterior surface, the animal expresses acute pain—but not when the anterior surface or middle part is touched or even torn. A very remarkable case presented to him by M. Royer-Collard, is well illustrated by these facts. A man, who had long been affected with paraplegia, and for seven years had been unable to move a joint of his lower extremities, nevertheless retained their sensibility unimpaired. After death, besides other less important derangements of structure, the corpora pyramidalia and olivaria were found reduced to the consistence of pap—and the softening extended, though with progressive diminution, along the whole anterior surface of the medulla, and throughout the whole thickness of its anterior cords—but the posterior surface was every where perfectly healthy,

The invention of M. Magendie, and the other spinal pathologists of the French capital, have been in vain put to the rack, to account for a very singular case which has lately occurred there to an accurate observer, M. Rullier. It is detailed in the

Journal of Physiology with great minuteness and apparent fidelity. We cannot spare room for a sufficient abstract of it, but shall merely give its most striking features, as contained in the reflections which Magendie has appended to the author's description. "A man, who enjoyed almost to the last hour the free use of his limbs, the sensibility of his arms, an acute understanding, and active generating faculties, had nevertheless experienced, probably for a long time, the loss of a full third part of the nervous tissue of the spine; the communication between the cervical and dorsal portions of the column was kept up, so to speak, by the membranes only—for nothing remained of the cord but a thin plate of white substance hardly two lines broad, and probably altered in its structure, while the vacuity from which the medullary matter had disappeared was filled with serosity. The upper and lower parts of the cord therefore were almost completely separated from each other by an interval of six or seven inches—yet the will governed the motions of the limbs, and the imagination stimulated the genital organs." The conjectures offered by way of explanation, are of course very far from being satisfactory, even in the eye of the Professor himself.—*Journ. de Physiologie, Avril*, 1823.

On the Formation of the Ovum in the Mammalia.—Dr. Plagge of Bentheim has been lately investigating this obscure and difficult subject, by careful observation of the ovaries in the larger mammalia. He completely agrees with the doctrine of Sir E. Home and Mr. Bauer, (Phil. Trans. 1818,) as to the formation of the ovum in the ovary before impregnation; but he thinks he has discovered, that instead of being formed in the corpus luteum, as these gentlemen affirm, the ovum, as well as the corpus luteum, is produced in the vesicles of Graaf; and that the corpus luteum bears the same relation to the ovum in the ovary, as the placenta does afterwards in the uterus. He observed, that a little areola first appears on the membrane of the vesicle—and not long afterwards, the rudiment of the future ovum may be seen like a grey speck on the middle of this areola, and the inside of the vesicle. After the rudiment has increased till its diameter is equal to three lines (in the cow), the corpus luteum begins to appear on the penduncle of the rudiment, betwixt it and the membrane of the vesicle, and the ovum is thus gradually pushed towards the surface of the ovary, to be impregnated.—*Journ. Complémentaire, &c. Avril*, 1823.

Effects of the Extirpation of the Kidneys. Discovery of Urea in the Blood.—MM. Prévost and Dumas, while endeavouring to ascertain the precise nature of the function of the kidneys, were led to observe the following singular effects, after these organs

were extirpated. One kidney may be removed from dogs, cats, and rabbits (which were the only animals operated on), with little immediate, and no ultimate inconvenience. Even the extirpation of the other produces at first but little injury—the wound even heals—but in three days the animal is seized with profuse vomiting and purging of a brown liquid matter—the pulse rises to 200, the breathing becomes rapid, the heat fluctuates to the 9th degree of Fahrenheit above and below the natural standard—and death takes place betwixt the 5th and 9th day. The inspection of the body shows the effusion of serum pretty abundantly into the ventricles of the brain, slight condensation of the lungs and mucus in the bronchi, more or less inflammation of the liver and distension of the gall-bladder, and great contraction of the bladder of urine. These phenomena have been formerly noticed in part by Haller and Richerand—but have never before been so accurately determined. Not long after the extirpation of both kidneys, Urea may be discovered in the blood, as appears from the following analysis. The blood was more serous than natural. Its serum and crassamentum being dried and boiled in water, the solution was evaporated, and the solid matter subjected to the action of alcohol. The residue, after the evaporation of the alcohol, weighed twice as much as that procured from the healthy blood of the same species of animal—*and concreted into a white crystalline mass when acted on by nitric acid.* This, when purified by a simple process, furnished scales of a pure pearly-white colour, being the nitrate of urea. From this important fact, viz. that urea exists in the blood, when the kidneys have been removed, MM. Prévost and Dumas have inferred, that "the kidney is nothing more than an *eliminating* surface like the skin, and that we are still ignorant where the urea and other principles of the urine are formed." Their experiments have been confirmed by Segalas and Vauquelin.—*Ann. de Chim. et de Phys. Mai,* 1823.

Transposition of the Viscera.—"I know not," says M. Scoutetten, "whether I have been favoured by chance, or whether visceral transpositions are more frequent than we generally imagine—but it is remarkable that I have met with three examples of the kind in less than a single year." All the subjects were young soldiers, who had passed their twentieth year, were of a good constitution, and enjoyed excellent health, till they were cut off suddenly by gastro-entiritic inflammation. In one of them the transposition of the chief viscera had been detected during life. The anatomical details present nothing remarkable, except the extreme precision with which the viscera of the opposite sides occupied the places of one another. The arteria innominata supplied the left arm and left side of the neck—yet

one of the men was well known to be right-handed. In all of them, "the vertebral column turned the concavity of its lateral curvature to the right instead of the left side"—a fact which Scoutetten thinks will contradict the notion of Bichat, that the ordinary curvature of the spine to the left side depends not on the situation of the aorta, but on the habit of using most frequently the right hand.—*Journ. Univers. Avril,* 1823.

MORBID ANATOMY.

Spontaneous Perforation of the Stomach and Oesophagus.—The Jour. Univers. contains an account of two good examples of this most inexplicable phenomenon. In one case, death arose apparently from sub-acute pneumonia, and corresponding appearances were found in the lungs—but the stomach was also perforated at its lesser curvature, and extensively eroded near the pylorus. In the other, the only symptoms observed during life, corresponded with the perforation of the œsophagus found after death, viz. pain at the root of the neck, difficult deglutition, and laborious breathing. The whole fore and back surfaces of the lower third of the œsophagus were destroyed, so that nothing remained but two lateral cords—and the stomach was entirely divested of its villous coat, and pierced at the middle with two holes, one as large as a half-crown piece. It is evident, that the former of these cases accords best with John Hunter's views of the nature of such perforations, while the latter rather corroborates the opinion of Professor Chaussier, who regards them as the consequence of a peculiar disease. The author of the cases, in a short but most pernicious commentary, ascribes them to the prolonged use of gin and corrosive sublimate!—*Journ. Universel, Mai,* 1823.

MM. Larrey *and* Roux's *Case of Excreted Intestine.*—In a report to the Académie Royale, upon a paper by MM. Bouniol and Rigal, junior, entitled Intus-susceptio intestinalis, followed by an excretion of about thirty inches of small intestine and a portion of mesentery,—MM. Larrey, Roux, and Beclard, state that the patient had been subject to great indigestion, and at last was affected with all the symptoms indicating internal strangulation. There were complete suppression of the alvine evacuations, stercoral vomiting, sharp pains in the abdomen, and an elevated tumour very sensible to the touch in the right iliac region. In the course of twelve days, at the termination of a violent fit of colic, he voided by the anus a portion of intestine and of mesentery. From this moment his amendment was rapid, his health was restored, and there remained no other inconvenience than a slight feeling of pain in the right iliac region. About

three months afterwards, the patient having eaten a very great quantity of cherries, was seized with symptoms of peritonitis and died—but the body was not inspected. M. Larrey thinks that a portion of the intestine must have been invaginated, strangulated, and seized with gangrene, separated in consequence from the living tissues, and thrown into the great intestines, whence it was ejected. In wounds of the intestines, M. Larrey has seen them re-united as neatly as if it had been done by the suture of a glover. Two pieces of intestine, indeed, unite most readily, as M. Larrey has demonstrated by experiments on dogs.—*Quart. Journ. of Foreign Med. and Surg.*

SURGERY.

On the Reunion of Wounds after great Operations.—M. Lisfranc, in a very elaborate memoir on a new mode of performing amputation at the hip-joint, has observed, that to prevent consecutive hemorrhage after this and other great operations, M. Dupuytren proposes to delay the dressing of the wound for two or three hours. Lisfranc himself has found, by many experiments on animals, and some trials on man, that when the wound is left exposed till the oozing of blood has ceased, and is then carefully wiped, immediate adhesion is much more frequent than after the usual mode of management. Hemorrhage, he adds, is always most hazardous after the wound has been dressed—time is lost in removing the dressings—they often adhere firmly to the wound, so as to cause more pain by their removal than the operation itself—and the clots deposited in the cavity of the wound, though M. Serres has found them sometimes organized, may nevertheless play occasionally the part of foreign bodies, hindering adhesion, and exciting dangerous inflammation. The practice here recommended has been for some time employed by several London Surgeons, and has also, we believe, been tried in this city. It is very favourably spoken of by all who have had recourse to it. A delay, however, of half an hour, or three quarters of an hour, has generally appeared quite sufficient for the oozing of blood to cease.—*Archives Gén. de Méd. Juin,* 1823.

On Bronchotomy in Croup.—The usual objection to the operation (proposed as it generally is, at too late a period of the disease,) is this—"That we cannot expect to succeed in making an opening *below* the point of obstruction, the inflammation and effusion of lymph having probably extended too far towards the lungs."

This objection rests upon the assumption that the effused lymph absolutely closes the air passages—but we believe that this is very seldom the case, and that the usual symptoms of

croup sufficiently prove that the false membrane formed in this disease acts as foreign bodies in the trachea in general do--causing death by exciting spasm of the glottis--and this being the case, a free opening below the glottis, although the cause of irritation may extend into the chest, might be expected to relieve.

We shall adduce some proofs of the positions above maintained, drawn from the best practical writers on the subject.

1st. With regard to the nature and extent of the obstruction in croup.

We believe that the effused lymph very rarely closes the air passages. On this point the opinion of Mr. Chevalier (who seems to have paid considerable attention to the subject) exactly accords with our own—he says, "I doubt whether death is often to be ascribed to such an accumulation of lymph as absolutely precludes the transmission of air."

If we consult the dissections recorded by authors, especially those by Cheyne and Jurine, we find that the following appearances have been observed:

An effusion of lymph lining the larynx and upper part of the trachea, sometimes extending as far as its division, and rarely into the more minute bronchial tubes—the consistence of this lining is various, sometimes it has not sufficient tenacity to admit of its being separated by the forceps or fingers from the membrane to which it adheres, sometimes it is so firm that it may be drawn out entire from the trachea, and when it has extended into the bronchial tubes it exhibits and long retains the aborescent form.

Besides this effusion of lymph in the inner surface of the lining membrane, there is often considerable swelling around it from the effusion of lymph on its external surface.

From the combination of these changes of structure, a considerable lessening of the diameter of the trachea must result; yet it does not appear that this is ever sufficient entirely to obstruct the passage, or even to reduce it to such a degree of narrowness as seems to have occurred in chronic cases some time previous to death.

We may remark, that the false membrane being sometimes so loose and moveable as to admit of its expectoration, a part of it may be carried upward and downward with every expiration and inspiration, the rest remaining fixed: it may thus act as a valve during expiration, and cause at intervals its entire and sudden obstruction. In general, however, after death, no part of the membrane is found so disposed as to be capable of acting as a valve, the whole of it adhering equally and firmly to the mucous membrane.

The bronchiæ and air cells, frequently also the trachea itself,

are found loaded with mucus, and this is often in such quantity, that some authors think it the usual cause of death; and certainly if such an opinion were well-founded, nothing could be expected from the performance of bronchotomy; but we have good reason to believe that this accumulation of mucus is merely a consequence of the original affection, which might be defined—inflammation of the lining membrane of the trachea, of the kind called adhesive, i. e. terminating in the effusion of coagulable lymph. This termination is a distinguishing feature of the disease—the effusion of mucus is not an immediate consequence of the inflammation—makes no part of the disease in question, but is merely a sequel of it, common to this and numerous other disorders of the respiratory system.

We are warranted then in concluding, from the dissections on record, that in general the effusion of lymph, the characteristic and exclusive produce of the inflammation in this disease, does but partially obstruct the air passage, although the accumulation of mucus which ultimately occurs perhaps in every case, may do so more completely.

The *symptoms* of croup are those which arise from a spasmodic affection of the glottis, combined with a certain degree of obstruction in the trachea and bronchiæ; and the characters which distinguish croup from severe catarrh or bronchitis in young children, are owing entirely to the spasm of the glottis.

The alternate remission and aggravation of the dyspnœa can depend on this cause alone, and the peculiar stridulous respiration is attributable as much to this as to the narrowing of the trachea, if not more so.

The occurrence of the chief symptoms of croup in paroxysms is acknowledged by the best observers; but it has received perhaps much less attention than it deserves in explanation of the nature of the disease, and in the direction of its treatment.

So prominent a feature is this intermission in many cases of the disease, that in the most classical work on the subject (that of Jurine) a distinct species is treated of under the name of INTERMITTENT CROUP

Taking it as an acknowledged truth, obvious to every observer, that the most urgent symptoms of croup are subject to alternate remission and aggravation, and considering that the effusion of lymph, if regarded merely as the means of lessening the diameter of the trachea, neither in any degree accounts for this *peculiarity* in the symptoms nor appears an adequate cause of their urgency, we are justified in concluding that the effusion has an additional and different influence.

The best established physiological principles—the impartial history of the disease in question—the history of diseases strikingly analogous, particularly cynanche laryngæa, and cases of

foreign bodies in the trachea—the effect of remedies—all concur in the indication that this additional influence of the effused lymph consists in an irritation of the glottis sufficient to produce its spasmodic contraction.*

The symptoms of this affection of the glottis frequently occur, however, so early in the disease, that the mere existence of inflammation previous to the effusion of lymph† must be sufficient to afford this degree of irritation. To the connection between inflammation and spasm, and the frequent examples of it in organs of a certain structure, we have already alluded when speaking of cynanche laryngæa.

According to the view we have taken of the nature of this disease, bronchotomy may give relief in it, although we should be unable to open the trachea below the point to which the false membrane has extended. This opinion may appear at first view absurd, it is perhaps novel, but it is still reasonable, unless the above statements on which it is founded can be pronounced utterly incorrect. The truth, however, of the proposition has yet to be decided by experience—and we can only now enquire how far the result of cases in which the operation has been tried hitherto, influences the question of its efficacy.

The operation has undoubtedly succeeded in many instances, but the success has been differently explained. In general it seems that the operator has figured to himself the trachea nearly, or entirely closed by coagulable lymph at a certain point, and has attributed his success to his having made an opening *below* that point; but what proof have we that this was really the case? No reason can be adduced why we should attribute success to this cause, and not to the mere fact of an opening having been made below the glottis, considering *that* as the real point of obstruction; it is as probable that in these successful cases the false membrane extended below the artificial opening as that it did not.

Mr. Chevalier, impressed with the conviction that the effusion of lymph was not such as "absolutely to preclude the transmission of air," ascribes the success of the operation in his cases to "its emptying the trachea of mucus;" but there is no proof that it was really owing to this cause, except that mucus

* Dr. Albers, of Bremen, seems to admit this opinion—he says, "We cannot deny that the spasm of the glottis, and perhaps also of the bronchiæ, may contribute to suffocation, as these passages are not always completely obstructed by coagulable lymph"—V. Abhandlung über den croup, Von Ludwig Jurine. Mit einen Vorrede und Anmerkungen herausgegeben, Von Dr. J. A. Albers.—*Leipzig.*

† Mr. Lawrence mentions a case in which the patient had well-marked symptoms of cynanche laryngæa, though after death the membrane appeared quite healthy, and the passage not at all narrowed.

was expelled through the artificial opening; may we not still ascribe the success, and with even better reason, to the cause we have mentioned, considering still the contracted glottis as the chief impediment to the transmission of air?

It may be objected to the view we have taken, that according to it the operation ought always to succeed, while there are but too many instances of its failure. We only maintain, however, that, contrary to the current opinion, the operation is calculated to give relief, that it may succeed even where we cannot make an opening below the limit of the false membrane. And we would argue also that the failure generally ascribed to the impossibility of making the opening in this situation, is obviously and justly ascribable to other causes; chiefly to its being deferred until, in consequence of long continued laborious breathing, and the imperfect influence of the air on the blood, these changes in the general system are induced, which (as Mr. Richter and Lawrence have so strongly insisted) are causes of death, even when the original obstruction may have been removed.

The accumulation of mucus, though no part of the original disease, yet occurs to such a degree in the latter period of it, that it becomes a principal source of danger, especially as at this period sufficient strength does not remain to admit of its expectoration. To these causes we would attribute the failure of the operation, when properly performed, rather than to the one commonly assigned—and if we are correct, we must conclude that the operation has failed, in general, only because it has been employed too late.

From what has been stated we would draw the following conclusions:—

1st. That the general nature of the disease, affords reason to expect the best effects from bronchotomy.

2. That these effects may yet be expected, when we cannot open the trachea below the limit of the effused lymph.

3. That where success has been obtained, it is to be ascribed merely to the introduction of air below the glottis, the real seat of the obstruction.

4. That the frequent failure of the operation may in general be ascribed to its being performed at so late a period, when the exhaustion of vital power, and the accumulation of mucus, which cannot be expelled, are sufficient causes of death, though the original obstruction should be entirely removed.

The above remarks are submitted to the profession, with the hope that they *may be* verified, and we would suggest as subjects of peculiar interest, the observation of the exact circumstances under which the operation succeeds or fails, and this with a view to determine the causes of failure and success so essential to

the right application of the operation, and certainly hitherto not well understood. It will not, we trust, be disputed that we are fully justified in trying the operation at an *early* period as often as opportunity offers.—*Quar. Jour. of For. Med. and Surg.*

M. Beclard *on Salivary Fistula.*—The difficulty of any salivary fistula is well known, and if it can be so ameliorated, as it appears to have been by M. Beclard's statement, we should have no hesitation in recommending its adoption. The method was to produce an internal fistula, to carry off the superabundant saliva. This was done in the case related by M. Beclard, by placing in the internal part of the thickness of the cheek a wire *(anse)* of lead, whose top fitted the excretory canal at the point where it was interrupted, and whose ends were tied in the mouth. The exterior wound, rendered bloody by incision, was united by the twisted suture. The cure was effected by M. Beclard in two cases without any deformity besides a linear cicatrix.—*Report of the Acad. Royale.*

M. Ferrier's *Operation for Staphyloraphia.*—A fisher, aged thirty-seven, had a stick driven backwards into his mouth in foolishly descending into a ditch, supporting himself on the stick with his teeth. It entered the velum, and was only arrested at the pharynx. M. Ferrier an hour after found that the wound of the velum presented a fragment *(lambeau)* in form of an angle of 70°, whose summit corresponded to the posterior spine of the nostrils—and the two sides were directed towards the free margin of the velum. The right side was nine lines in length, and the left twelve. The fragment fell down upon the base of the tongue. The back part of the mouth was observed through the wound. The tone of the voice was altered. Upon trying to swallow some coffee, part of it escaped through the nostrils. The indication of the treatment was plain—but the choice of means difficult. The interrupted suture was insufficient. M. Ferrier executed the operation thus: he placed the patient opposite to a window, his head supported and fixed on the breast of an assistant behind. His mouth was kept open by a piece of cork, fixed between the teeth of the right side. M. Ferrier then took a pair of polypus forceps in his right hand, between whose blades was fixed a flat semicircular needle, six lines in diameter and armed with a ligature, whose ends were a third of a metre in length. The direction of the needle stood at an angle of 90° with the blades of the forceps. The index and middle finger of the left hand in a state of supination, were placed upon the base of the tongue, immediately under and supporting the torn fragment. He then, with the needle pierced the fragment from behind to before, three lines from its point,

in such a manner as to make the needle pass between the two supporting fingers. When he had thus passed the needle he disengaged the forceps, carried them below the supporting fingers, and seized with them the point of the needle, and carried it out of the mouth, leaving the two ends of the ligature traversing the torn fragment, when it was no longer supported by his fingers. He then separated the needle from the ligature, and fixed on it the pipe of a small quill six lines in length, and forming with it a true T. He did no more than pass by the nostrils the ends of a thread opposite to that to which the quill was attached, an operation easily performed by means of Bellocq's sound, passed into the left nostril. The spring of the instrument being pushed, entered the mouth by passing through the back part of the nostrils and the wound—and to this spring the ends of the thread were fixed. On retracting the sound, it brought with it the threads, raised up the torn fragment, and maintained it in its place. He then separated the threads at the anterior nostril and fixed them by means of a knot and a loop upon a roll of linen placed before the opening. The patient was forbidden to speak, and had only pea soup for four days. Every day the threads were examined to see whether they remained sufficiently tight. On the fifth day, the torn fragment having formed a sufficient adhesion, the quill was removed with the rest of the apparatus. On the eighth day the patient was well and returned to his usual food.—*Revue Medicale.*

M. Dupuytren's *Case of Wry Neck.*—When this deformity is occasioned either by spasmodic contraction of one of the sterno mastoid muscles or paralysis of the other, it may sometimes be relieved by an operation. In a case of the former kind it would be requisite to divide some of the fibres of the diseased muscle; in the latter, a sufficient quantity of the corresponding healthy one would require to be cut, in order to establish a uniformity of action between the two.

The history of the following case may serve as a guide in practising the operation, as well as one proof of its success.

A little girl about ten years of age, whose neck, or rather her head, had been awry for three years, owing to a permanent spasmodic contraction of the sterno mastoid muscle of the right side, was admitted into the Hôtel Dieu, Paris, early in January 1822. On the sixteenth of that month the operation was performed by M. Dupuytren as follows.

The patient reclining against an assistant, a puncture was made, with a straight narrow bladed bistoury, through the integuments just on the inner border of the sternal extremity of the contracted muscle. The blade of the bistoury, being flatly op-

posed to the muscle, was pushed cautiously behind it, the point being directed forwards and outwards till it protruded just on the outer side of the clavicular border. The edge of the bistoury was then turned towards the muscle, and a sufficient quantity of its posterior fibres cut to allow of the head being placed erect: the instrument was then withdrawn.

In this way the integuments escaped being divided, and a future scar was prevented; a very desirable object, the patient being a female.

The cut edges of the muscle were kept asunder by depressing the clavicle, and inclining the head to the left side. The former was effected by binding the right hand firmly to the foot, the knee being bent; thus the clavicular fibres of the deltoid drew the bone downwards—the latter by a roller passed round the head and under the left axilla.

The patient was kept in bed—and at the end of thirteen days the punctures were healed, and she had free motion of the neck, though from long continued habit, she still turned her face to the left side. The bandages were re-applied, and the same bodily position maintained till the 21st of February, when they were finally taken away, and the patient pronounced cured, the head being but very slightly inclined to the right side, and having free motion in every direction.

In operating on the male, the fibres may be cut on the anterior surface of the muscle, an incision being first made through the integuments. Inclining the head to the opposite side by a roller, and filling the wound with lint, will then be sufficient to keep its cut edges asunder.—*Quar. Jour. of For. Med. and Sur.*

Hernia through the Diaphragm.—M. Jules Cloquet has related a case of this nature of recent occurrence. The subject of his observation was a man of about forty-five years of age, of healthy appearance, who had the thorax strongly compressed from before backwards, between the wheels of two carriages. The patient, who was carried to the hospital of St. Louis immediately after the accident, complained of intolerable pain in the chest. The respiration was difficult and interrupted—the pulse accelerated and intermitting—the face swollen, of a purple colour, and having a very peculiar expression of pain. Antiphlogistic remedies were employed in their fullest extent—nevertheless the patient died thirty-six hours after the accident had occurred. On examining the body the soft parietes of the thorax presented no lesion, but several ribs were fractured—the diaphragm was considerably torn at the left side, the rupture extending towards the centre—the pericardium was lacerated in all its anterior and inferior portion—the whole of the stomach and the greater part of the colon had passed into the left cavity

of the chest, and was situated in immediate contact with the heart and lung—this last was pressed back upon the vertebral column. The cavity of the pleura was filled with blood.—*Revue Medicale,* Juillet.

Swelling of the Tongue cured by Incisions.—A woman, aged twenty-eight years, was exposed to cold and anxiety of mind during menstruation, by one or both of which means the discharge was suddenly much diminished. To this supervened a violent affection of the tongue, which impeded deglutition so much as to induce her attendant to order the application of fifteen leeches to the labia, bleeding from the foot, and an irritating enema. Notwithstanding these remedies the tongue became so much swelled as to press backwards into the pharynx, and backwards out of the mouth between the teeth—suffocation under these circumstances became imminent. Respiration could only be performed through the nose—the evacuation of the sputa was extremely difficult. Thirty leeches on the neck produced only very temporary relief—when the surgeon (M. Faneau de la Cour) made two deep incisions from the root to the point of the tongue, after the manner recommended by De Lamalle, in his memoir presented to the Royal Academy of Surgery in 1752, and by Job-a-Meekren, a Dutch surgeon who had practised the operation in 1656. Considerable diminution of the tongue resulted, which however remained much larger than in its natural state. An emetic of tartarized antimony completed the cure. It is worthy of remark that in some of these instances of violent swelling of the tongue, the disease only affects one side.—*Bulletin de la Societé Medicale d'Indre-et-Loire.*

Artificial Palate.—The natural want or the casual destruction of that delicate organ, the human palate, is attended with the most unpleasant of all effects—the loss of voice; and of the the many substitutes which we have, very few have those advantages which could be wished. The common metallic palate seldom fits well, and always gives pain—while those of gum caoutchouc and other elastic substances are offensive, and also, by pressing asunder the parts, increase the deficiency. The removal of them all, for the purpose of cleaning, is a work of some trouble. We have seen a silver palate, constructed by Mr. Clark, of Grosvenor-street, a very ingenious dentist, which obviates many of the objections to the old construction. It fits the parts with the utmost nicety, and as it does not at all press upon the edges of the deficiency, it allows the parts to contract or even to be to a certain extent reproduced—while the wearer can take it out, clean it and replace it, in two or three minutes. When it is to be removed or put in, the wings which fasten it

to the upper side are made to collapse into a very small space—and after it is put in its place, they are made to expand and embrace the edges of the bone with any degree of tightness that may be necessary. The whole of the machinery (which is very neat,) is worked by a small button in the centre of the palate, so flat as to give no uneasiness to the tongue, and yet which can be moved with the greatest ease. Besides the facility with which this palate can be removed and replaced, the great advantage of it consists in the accuracy with which it fits the parts. The inventor being an expert worker in metals, cuts and works the whole himself—and by this means is enabled to procure a perfect model, and also fit it precisely. In some very bad cases Mr. Clark has fitted those palates with joints, by which they could, without any pain to the wearer, be made fast to the moveable parts of the mouth. It gives us pleasure to notice this invention, not more on account of the merit of the inventor, than of his modesty. It is four or five years since Mr. Clark made and fitted the first palate of this kind—surgeons of the first consequence in town and also in Edinburgh, have spoken highly of it—and yet the inventor has not, in the usual way, called the attention and claimed the patronage of the public.—*Lond. Med. and Phys. Journ.*

MIDWIFERY.

MALACARNE'S *Case of Laceration of the Pregnant Uterus.*—The subject of this accident, aged 27, and mother of three children, was in the fourth month of pregnancy. On April 25, 1812, she had suffered much fatigue during very bad weather. On returning home she complained of pain in her left side, which was succeeded on the following morning by nausea, lasting some days, and by febrile symptoms. These continued until August 3, when she was seized by violent agitation of the whole body, tumour of the belly, and insupportable pain in the side. A physician called to her found her in a state of syncope; she recovered and did not complain of much pain, but became convulsed when the abdomen was touched. The face, which had been flushed, became pale—the eyes were sunk in the head—the pulse languid and intermittent—viscous sweats covered the body—syncope occurred alternately with convulsions, and only terminated with life.

On opening the body the omentum was found raised by much coagulated blood diffused among the intestines, and in the midst of it was discovered a fœtus, which, from its size, was judged to be about four months The blood being removed, and the intestines turned aside, the uterus presented itself somewhat flattened, and with an irregular laceration, from which hung some folds

of a flaky, fleshy, irregular substance, still pouring out blood. The laceration was on the convexity of the uterus, nearer to its longitudinal axis than the attachment of the Fallopian tube and ovarium, which could not be discovered. The writer supposes the laceration to have been the consequence of previous disease, and death to have ensued from the profuse hemorrhage thus occasioned.—*Mem. dell' J. R. Istitut. del Regn. Lombard.*

M. Meirieu *on Rupture of the Umbilical Cord.*—M. Chaussier, and others, having doubted the possibility of the rupture of the umbilical cord, by the mere weight of the child, M. et P. Meireu presents us with a case of a Madame P. thirty-five years of age, in good health, and pregnant with her third child, who was struck by the pole of a coach on the left side of the abdomen, in the eighth month of her pregnancy. She, however, was not taken in labour until the conclusion of the ninth month, and was delivered of a female child. A few moments before this event, Madame P. was walking about her room, and was seized with a strong pain; she took firm hold of the bed-post, brought herself nearer to the ground, retained the infant by means of her clothes, and placed it on the floor. The whole was the affair of an instant. One of the assistants took the child, aud found the umbilical cord broken. This was before the arrival of Dr. Meirieu, who on examination, found not the slightest traces of contusion on the child; it had, however, a spina bifida, occupying the lower part of the loins and the upper part of the sacrum. The umbilical cord was separated about four inches from the ring, and the end drawn out to a point.—*Journal Universel.*

M. Mondat *on Impregnation by the Aura Seminalis.*—Our author regards the aura seminalis as the active part of the semen, and quotes, in support of his opinion, the following decisive experiments performed at Turin by himself and two other physiologists. The semen of a dog having been received into the cup of a funnel bent for the purpose, its tubular part, ten inches long, was thurst three or four inches into the vagina of a bitch in heat, to convey to the uterus the aura seminalis. In eighteen out of thirty trials, impregnation was produced; and the same result was obtained upon two mares.—*Journal Universel.*

THEORY AND PRACTICE OF MEDICINE AND MATERIA MEDICA.

On Partial Dropsy.—M. Bouillaud, in a memoir on obliteration of the veins, has endeavoured to show that this accident is the sole cause of partial dropsy of the passive kind. He forti-

fies this opinion by first relating three cases of dropsy of both limbs, arising from obstruction of the vena cava, or both iliac veins. In two of these cases, the obliterating matter consisted of fibrinous coagula, conjoined in one with a cancerous tumour of the rectum, uterus, and adjoining parts—in the other, with fungoid enlargement of the ovaries. In the third case, the matter filling the veins resembled the medulla of the brain, and was connected with fungoid enlargement of the right kidney. He next details four cases of dropsy in one limb, arising from obliteration of its crural or iliac vein, by the deposition of a solid, reddish, fibrinous, friable matter. And finally, he describes two cases of pure ascites, uncombined with anasarca, in both of which, the vena portæ was choked up with a fibrinous clot, and compressed by a tubercular swelling of the liver. M. Bouillaud adds, that the obliteration is generally the consequence of simple pressure, though, in three of his cases, he suspects it to have been caused by inflammation. From these data he ventures to infer, that general dropsy of the passive kind likewise arises from obstruction to the course of the blood through the veins, and not from general debility, as is commonly thought; and in support of this assertion he alleges, that it is observed only in the advanced stage of diseases of the heart, thoracic vessels or lungs, when the venous circulation is embarrassed. We believe he is quite correct in attributing partial passive dropsy to the cause assigned—but the propriety of extending the explanation to general dropsy may be questioned.* M Bouillaud has been eminently fortunate in witnessing a number of most valuable and apposite cases—and to these he has annexed a short notice of some others to be found in books. His paper, however, is to be considered rather as expressing the general opinion and experience of the profession, than as advancing any new doctrine, or grand fact in pathology—for the connexion of passive partial dropsy, with obstruction of the veins is, we apprehend, familiar to all British practitioners at least.—*Archives Gén. de Méd. Juin,* 1823.

SEGALAS D'ETCHEPARE *on the Action of the Chlorate (Oxymuriate) of Soda on several Animals.*—This substance has already been proved to be very efficacious in destroying infectious effluvia. It has also come into use in several hospitals, and even amongst some practitioners as a detersive application to ulcers of different kinds, and particularly to those of a venereal nature. Several experiments on dogs have been made to

* We may refer M. Bouillaud, or those who think as he does, to a curious paper by M. Gaspard in the 1st volume of Magendie's Journal, on an Epidemic Anasarca, which ravaged several departments of France after the bad crop of 1816. It was evidently *passive,* and clearly depended on general debility.

prove its power over the animal economy. The effusion of a dram of the solution on the denuded pectoral muscle caused severe pain. An ounce of the liquid injected into the stomach of a large dog produced immediate vomiting, &c. so as to reject the whole of the substance. Two ounces of the liquid were then introduced into the stomach of the same animal by an opening made in the œsophagus, and retained by a ligature —he made repeated efforts to vomit. Having survived the operation, he was killed twenty-four hours afterwards. The stomach was found inflamed throughout with the exception of a small portion near the pylorus.

In some of the animals killed, the veins were filled with blood of a less deep colour than usual, and there was a strong disposition in the muscles to contraction. The conclusions from these experiments are, first, that the chlorate of soda ought to be reckoned amongst the class of irritating and corrosive substances. Secondly, that the solution (the solution employed as one marking twelve degrees of the aërometer) besides its direct and sympathetic action on the solids, exerts by absorption a manifest action on the blood. Thirdly, that it ought to be used with extreme caution on denuded surfaces, and especially on the parts of generation.

The chlorate of soda is rarely employed pure, but diluted with six or eight parts of water, when it becomes much less irritating.—*Magendie's Journ. de Physiologie.*

Fatal Hemorrhage from Rupture of the Fallopian Tube.—A woman who lived on bad terms with her husband, one evening, during a violent quarrel, threw a chair at him with all her force. Early next morning she was seized with violent colic and frightful vomiting and purging. These symptoms were followed by swelling of the belly—and she expired in the midst of horrible convulsions, after thirteen hours illness. Not long afterwards a suspicion arose of her having been poisoned—the body was disinterred ten days after death, and carefully examined by order of the proper functionaries. It was quite fresh, without lividity or contusions. All the organs in the chest and head were healthy, and so was the whole tract of the alimentary canal—but the belly was distended with serous fluid and enormous clots of blood, amounting to more than eight pounds. After a deliberate and careful search, the dissector at last found that the hemorrhage had proceeded from an oblong, irregular, fringed perforation, an inch in circumference, situate on the right Fallopian tube, near its attachment to the uterus. He does not appear to have searched for the torn vessel or vessels which had yielded this enormous quantity of blood. As the woman was nursing at the time of the accident, it is very likely that the

tube ruptured was the one through which the last ovum had descended, since it preserves its tenderness and great vascularity for some time after delivery.—*Journ. Univers. Avril*, 1823.

Pathology of Palsy and Apoplexy.—M. Serres has commenced a series of interesting papers in this Journal, on the relation subsisting between the chief symptoms of these diseases, and the seat which the corresponding organic derangements occupy in the encephalon. He has found, by referring to authors, and also by frequent personal experience, that in apoplectics, when erection of the penis is a symptom during life, or is found in the dead body, the apoplectic cell is always deep in the cerebellum; that in paralytics the loss of motion of one arm alone is connected with derangement of the posterior lobe and posterior part of the middle lobe of the brain, and paralysis of one leg with a deposite either among the anterior radiations of the corpus striatum, or in the cerebellum—and that in every case of hemiplegic palsy without exception, the cell occupies the opposite side of the brain or cerebellum. It is a prevalent idea, he observes, that while an injury of a hemisphere of the brain causes palsy of the opposite side of the body, that of the hemisphere of the cerebellum paralyzes the same side. This idea, however, he finds to be erroneous—for in three cases of marked hemiparaplegia, he discovered a purulent cell in the opposite side of the cerebellum; and in numerous experiments on animals he invariably remarked, that an injury of a hemisphere or peduncle of the cerebellum, impaired or destroyed the power of motion in the opposite side of the body. He allows that the reverse has been witnessed by others, and has the candour to quote against himself the respectable authorities of Helvetius, Plancy and Forestus. But he overcomes this difficulty by assuring us, that several years ago, he did not hesitate to pronounce all their observations erroneous, and that he expects to prove them so to every body's satisfaction.—*Journ. de Physiologie, Avril*, 1823.

Desmoulins *on the Discolouration of the Skin in Yellow Fever.*—M. A. Desmoulins has laid before the Institute a paper on this subject, in which he has come to the following important conclusions:—

1st. That there is not in yellow fever any encreased secretion of bile. 2d. That both the black coloured substances vomited and evacuated per anune, are exhalations from the coats of the intestines. 3d. That the yellow colour of the skin takes its rise from an elaboration of the blood, in the corpus mucosum of the skin, in which a sanguineous congestion is established by a determination, simultaneous with, and analogous

to, that which produces the hemorrhage from the mucus membrane of the intestines. 4th. That the more dense structure of the cutis is the only reason why hemorrhage does not take place from it. 5th. That the yellow tinge of the skin is merely a species of ecchymosis. 6th. In a word, that the yellow fever is nothing else than a determination of blood to the skin and mucus membranes, the effects of which are diversified on these surfaces by the different degrees of the intensity of the determination combined with the unequal permeability of the membranes.*

These different propositions are supported by a number of ingenious, and in many cases, conclusive arguments, of which our limits will only admit an outline. In several cases where the black vomit had occurred, the stomach after death was found filled with the same matter, while the pylorus was entirely obstructed by scirrhus, proving that water could not come from the liver—nay, Dr. Ffirth discovered this dark substance completely formed in the arteries of the stomach. Authenrieth, and several others, have observed the serum to be yellow in diseases free from any biliary complication--in the bodies of children who were born with the yellow gum, no indications of hepatic disease could be discovered, nor in a case of this kind, examined by M. Lassaigne, could the least trace of bile, or any of its elements, be found either in the serum or the fibrine, or in the coloured particles of the blood. Some old men have become yellow, and yet enjoyed good health—and some nations have a permanent yellow tinge. This colour cannot, therefore, in all cases be the effect of bile, and in the yellow fever is most probably owing to the elaboràtion of the blood in the corpus mucosum Malphigi.

In conclusion, M Desmoulins thinks he can perceive a conformity of the symptoms of yellow fever with those in the diseases produced in dogs, in the experiments of M. Gaspard, by the transmission into their veins of the fœtid juice of fermented cabbages. This resemblance he traces still farther, viz. to the symptoms on dissection, and from thence is led to deduce a similarity of origin in both diseases, namely, the introduction of putrid substances into the mass of blood. This analogy of symptoms and origin, of course, extends not only to yellow fever, but to typhus, to intermittents from malaria, and to all diseases supposed to take their origin from putrid exhalations.—*Magendie's Journ. de Physiologie.*

Mr. Swan *on the Causes of Apoplexy.*—In giving the history of

* We have taught, uniformly, these very positions in our lectures. Most of them may be found in Warren's work on the Yellow Fever of Barbadoes, published nearly ten years.—Editor.

four cases of apoplexy, in which emetics appeared to have been exhibited with advantage, contrary to the very general opinion of this practice being dangerous, Mr. Swan takes occasion to remark, that apoplexy depends more frequently on the difficult transmission of blood through the lungs, or from its not being properly organized, than is imagined. A stomach overloaded by wholesome food, or containing a small quantity of indigestible food, deranges all the parts to which the branches of the par vagum are distributed—and therefore the lungs do not perform their functions; in which cases, though bleeding is absolutely required, the quantity of blood may be taken away without affording complete relief—therefore, if a person is seized with apoplexy, and bleeding has not afforded relief, and there is no symptom of paralysis, and it is probable the stomach contains undigested food, it is advisable to give an emetic.—*Edinburgh Journal.*

M. Dommanget *on Small-Pox.*—Dr. Clutterbuck will be *ravi* to see that M. Dommanget's dissections tend to support his theory of fevers. In several cases of small-pox, M. Dommanget found the membranes of the brain either inflamed, in a state of adhesion, and injected or covered with effused matter. The brain itself was either softened or its substance injected. The lungs were sometimes hepatized and sometimes engorged. There were adhesions and inflammations of the pleura with albuminous affections—and the stomach and intestines were usually inflamed.—*Journ. Général.*

On the Therapeutical Effects of Iodine. By Dr. Coster.—When reviewing, in the Archives Générales de Médecine an Essay on Iodine, by Brera, entitled "Saggio Clineo sull' Iodio," Dr. Coster has offered some remarks on the successful employment of this substance, in some cases which he observed in the practice of Dr. Coindet, of Geneva.

"Particular circumstances," says Dr. Coster, "having led me to remain for eight months at Geneva, with this distinguished Practitioner, (Dr. Coindet,) I was enabled to observe accurately the good effects of iodine in enlargements of the thyroid gland, and in scrofulous tumours. M. Coindet first of all employed this medicine under the form of alcoholic tincture, and obtained very surprising effects from its administration in goître. It was not long before it was perceived that iodine did not act solely on the thyroid gland, but that it diminished the form and size of the mammæ. In some imprudent patients, who under the idea that their cure would be hastened by it, secretly exceeded the dose prescribed by the Physician, it produced a degree of irritation which induced marasmus.

"M. Coindet next tried the employment of iodine in friction on the tumour itself—and the success from the application was so great, that of nearly one hundred individuals affected with gôitre, whose cases I have collected, I may affirm that more than two-thirds were completely cured by it. The hydriodate of potass incorporated with lard was the form under which the iodine was first administered in frictions.

"Soon after these successful results, the iodine was employed in scrofula, in the same manner, viz. sometimes internally, and sometimes in the form of friction. I shall not affirm that the success was as constantly favourable in this latter disease as in the former, but it is certain that scrofulous tumours yield sooner to the action of iodine than to that of any other remedy at present known. When the tumours, whether of the thyroid gland, or of the lymphatic glands, are hard and renitent, experience proves that the effects of iodine are much more prompt, when the frictions are preceded by the application of leeches, and by submitting the patient to a demulcent regimen. Notwithstanding these precautions, however, it occasionally happens that the tumour remains stationary, although it may not be of the nature of those which are acknowledged to be incurable. A case of this kind having occurred to me, I tried the following experiment, which I merely relate here for the purpose of inducing Practitioners to repeat it.

"A young man was affected with a gôitre, the size of which was, at least, equal to that of three pullets' eggs: it had been unsuccessfully treated, in the first instance by iodine in frictions, afterwards internally by the same substance, and finally by leeches, followed by fresh frictions. Some one gave me the idea of combining the action of the voltaic pile with that of the iodine, and it is known that the positive pole of the pile exercises an attractive action on iodine. From these data, I conceived that by making use of frictions with pure iodine on one side of the tumour, and applying the pole on the opposite side, the absorption would be more speedy, and the effects of the iodine on the tumour more sensible. In order, however, not to attribute to the iodine those effects which might be believed to be those of the electric action, I began by subjecting the patient, for eight successive days, sometimes to the stream of the pile, and at others to the action of sparks; but it was all in vain. I then began with the experiment. The tumour of the thyroid gland was placed twice a day, for the space of ten or twelve minutes, under the influence of the positive pole of the pile, taking care to change sides each time of using it—so that in the morning I made use of friction on the right side, and the action of the pile on the left—whilst in the evening I chose the opposite sides. At the end of four days, the size of the gôitre had

diminished about four lines. On the tenth day it was reduced to one third, and at the end of twenty days there did not remain the least trace of it.

"The quantity of iodine which I employed, was two grains to a scruple of lard. During the whole of this treatment no unpleasant symptom occurred—the skin had, however, a violet tint, but this disappeared in four or five days. I regret not having been able to repeat the same experiment, as no isolated observation is sufficient to establish a fixed rule."

M. Coster considers that this plan might possibly be successfully employed in scrofulous cases—but he agrees with M. Coindet, that if the lymphatic irritation become raised to the inflammatory state, which is marked by heat and redness in the tumefied glands, the iodine quickly occasions suppuration, which in scrofulous cases it is essential to avoid, from the cicatrix which is always produced by it. Under such circumstances he considers it prudent to abstain from the use of iodine.—*Archives Générales de Médecine, Juillet,* 1823.

The Efficacy of Oil in Cases of Poisoning by Potass. By M. A. Chereau.—Case 1st—The grand-daughter of a copper-founder, when very thirsty, drank a glassful of a solution of potass used for scouring metals. The proportion of this alkaline solution was about sixteen ounces of red American potass to a little more than two pints of water. The liquid was of a reddish colour, which at first induced a belief that it was a mixture of wine and water, which the workmen were using—she drank it with avidity, but soon experienced great heat in the throat, with violent pains in the stomach and abdomen. M. Chereau being called in, made her take at once ten ounces of syrup of gum, and four pounds of oil of sweet almonds. Vomiting supervened—and when it seemed to stop, the mixture was repeated: the vomiting after this was renewed, and the patient felt herself relieved. Emollient drinks and oleaginous potions, administered by spoonsful, subsequently removed all the alarming symptoms.

Case 2d.—The child of a wine merchant had drank by mistake some of the *aqua secunda* of painters (a solution of potass in water.) Nearly three pounds of oil of sweet almonds were administered at two different times, and relieved the unpleasant symptoms. Vomiting occurred as in the first instance. It was remarked in this case, that the rejected liquid was of a whitish colour, with some density, and of a saponaceous appearance. It would seem that the oil does not only act in provoking vomiting by the nausea which it occasions, but also that it combines with potass in the stomach.—*Bulletin des Travaux de la Société de Pharmacie de Paris. Juillet,* 1823.

On the Use of Sugar in Cases of Poisoning by Lead. By M. Reynard.—The following fact has been lately addressed by M. Reynard to the *Société des Sciences* of Lisle. During the campaign of Russia, several loaves of sugar had been enclosed in a chest containing some flasks of extract of lead. One of these flasks having been broken, the liquid escaped, and the sugar became impregnated with it. During the distresses of the campaign, it was necessary to have recourse to this sugar—but far from producing the fatal accidents which were expected, the sugar formed a salutary article of nourishment to those who made use of it, and gave them a degree of vigour and activity which was of the greatest service in enabling them to support the fatigues of marching. "Might not," says M. Reynard, very properly, "the employment of sugar be adopted for combating the effects of the sub-acetate of lead, instead of the other soluble salts recommended by M. Orfila, (the sulphate of soda or of magnesia,) which are not always ready at hand."—*Journal d'Agriculture et Sciences accessoires.* Par M. Limousin-Lamothe. Tom. i. No. 4, page 94, 1823.

M. Andral, Jun. *on the Medical Properties of Strychnine and Brucine.*—The strychnine used in the cases which follow is as pure as possible, and is made into pills containing either a twelfth or a sixteenth of a grain.

Of the former of those, one was given night and morning to a painter who had paralysis, after several attacks of colic. A sort of painful trembling was produced in the extensor muscles of the hands. The dose was encreased to four grains a day, which created spasmodic contractions of the extensores digitorum; the paralysis seemed to diminish, and after a short time he quitted the hospital, having only a slight weakness left in his hands. The dose could not be exhibited to this individual without danger beyond two-thirds of a grain per diem.

A single pill, containing only a twelfth of a grain, given to a grinder of colours, with the same disease, produced a slight trismus and the commencement of tetanic rigidity in the muscles of the neck and abdomen. At the end of six days, two pills were given, which occasioned violent contractions of both arms. This dose, continued about fifteen days, completely dissipated the paralysis.

It required a third of a grain to produce any effect on a German with a paralysis of the extensores of both hands. The dose was increased to above a grain, which created powerful contractions, so that it was lowered to a grain. No benefit occurred to this patient, but the case serves to mark the different susceptibility of patients to the action of this substance.

To a potter the strychnine was given to the extent of a grain

without much effect—on passing that quantity there came on lock-jaw and retraction of the head. He would take no more pills, and went out without much benifit.

In another case the paralysis seemed to arise from lesion of the spinal marrow, and was aggravated by the strychnine.

The brucine was administered in pills containing half a grain of the alkali. In a paralytic grinder of colours, one pill produced no effect, but four pills caused two slight agitations, and strong contractions. He was cured.

Another took four grains without any sensible effect, but five grains excited pretty strong agitations. The paralysis was much alleviated.

A painter and plumber, who both received the medicine, reaped little amelioration from it. In the former, three grains gave rise to pretty violent trismus; and in the latter, three grains and a half occasioned tetanic rigidity of the limbs.

The conclusions from the above cases are, that strychnine has a similar action on man to the extract of nux vomica, but in a more intense degree. Brucine acts in a much less intense manner, and may therefore supersede the alkali of nux vomica. With regard to their curative qualities, both strychnine and brucine are more or less efficacious according to the species of paralysis. Where this affection is connected with an inflammatory state of the brain or spinal marrow, it most probably will aggravate the symptoms. Where it is the result of apoplectic attacks, they are also often useless, but to the paralysis which occurs without any lesion of the brain or spinal marrow, such as is usual in those who work with preparations of lead, these alkalies are an efficacious remedy.—MAJENDIE's *Journ. de Physioligie.*

On the seat of Extravasation in Hemiplegia.—Messrs. Feville and Pinel-Grandchamps give the following account of an old woman, who died at the Salpêtrière. She had been affected for many years with complete paralysis of the upper and lower extremities of the left side. The remains were found of an old extravasation of blood in the right hemisphere of the brain, in the medullary space between the optici thalami and the corpora striata, involving both these parts equally. Messrs. Feville and Pinel-Grandchamps laid this case before the Academy of Medicine, as tending to confirm their former observation on the seat of movement in both members. They are of opinion that the seat of motion of the upper extremity is situated in the optici thalami, and that of the inferior extremity in the corpora striata. In a case where the arm and leg are both struck with palsy, the optici thalami and corpora striata must, according to their views,

be both alike disorganized. In the number of Majendie's Journal for April, M. Serres likewise maintains the opinion that the cause of the movements of the upper extremities resides in the posterior part of the hemispheres, and the cause of movement of the lower limbs in the anterior part of the brain.—*Revue Medicale, Juillet.*

CHEMISTRY AND PHARMACY.

Spontaneous Combustion.—Though this singular incident has ceased to be a novelty, yet the following case is so striking, chiefly on account of the evidence establishing it, that a short notice of it may not be unacceptable. In almost every circumstance it agrees perfectly with those which have been related by former observers. The particulars were collected under legal authority by MM.Colson and Lelarge, practitioners at Beauvais, where it happened. The subject was a very tall fat brewer, long addicted to excessive drinking. At twelve on the evening before the accident, a woman of the house ascertained that he was in bed, and his candle extinguished. Next morning at eight, a thick smoke being seen to issue from his apartment, the door was burst open, and his body found stretched on the brick floor and still burning. The flame was extinguished with difficulty by the copious affusion of water. Close beside the body stood an iron pot containing a little half consumed charcoal, and near it a straw-bottomed chair lay upset, with its bottom burnt away. Except the body and the chair, nothing else in the room was injured. The flesh of the neck was destroyed, except on the anterior part. The whole left arm and the thoracic parietes of the same side were entirely consumed—and of the right side and arm, nothing could be seen but some charred fragments of ribs, and the shoulder, the humerus, and part of the fore-arm. Both thighs and the right leg were likewise almost entirely consumed; and of the viscera of the trunk, the only remnants were the heart, lungs and liver, which, however, were all more or less torrefied. This account affords no satisfactory evidence of the spontaneous inflammation of the body—but it clearly establishes the fact, that it may acquire a preternatural combustibility.

Another case, which occurred in the neighbourhood of Bordeaux in September, 1822, has been related in the last volume of the Nouveau Journal de Médecine; but though the particulars were sworn to before a magistrate, our readers will not be at a loss to discover good reasons for doubting its authenticity. A sober healthy blacksmith was returning home in company with a girl one very hot afternoon, when he suddenly felt an acute pain in the right index, and was astonished to behold it burning and smoking. He rubbed it against his thumb to extinguish the

flame, but the flames caught both the thumb and middle finger. He then rubbed them on his trowsers, but burnt two holes in them; next he thrust his hand into his pocket, and set it on fire too; and finally, he happened to touch the fore and middle fingers of the left hand, when these caught fire also. In vain did he plunge them into a bucket of water—they continued to burn. In vain did he stick them in the mud; the virtues of the mud were not more potent. At last, a devout female reminded him that the faith saves us, he dipped them in holy water, and the flames were speedily extinguished. Since the well known story of the priest Bertholi (see Foderé Méd. Lég. III.210.), this is the only instance of alleged spontaneous combustion where the sufferer has been seen during life. The most amusing part of the whole story is, that the relater doubts none of the circumstances except the efficacy imparted to the water by its sanctification.—*Journal Compl. du Dictionnaire, &c. Juin,* 1823.

Blue-coloured Urine.—M. Jules Cloquet relates an instance of a boy of thirteen years old, who during three successive days, in the course of a severe attack of enteritis, voided urine of a pure blue colour, loaded with sediment of the same appearance, which communicated a beautiful indigo tint to paper. One of the members of the Academy who was present during the relation of this case, mentioned his having once seen a similar phenomenon in the person of a man affected with acute rheumatism.—*Revue Medicale. Juillet.*

Vegetable Alkali from Rhubarb.—M. Nani, of Milan, states that he has discovered a new vegetable alkali in rhubarb—but he has not said much of its properties, and except its solubility in weak sulphuric acid, and precipitation by lime, no evidence of its alkaline nature is offered. Six ounces of rhubarb in powder were boiled for two hours in eight pints of common water, with four drachms of sulphuric acid—this was filtered, pressed, and the residuum reboiled with six ounces of water and two drachms of sulphuric acid—the fluid being again separated, the residuum weighed but two ounces. The united infusions were mixed by degrees with three ounces of quicklime, and from being yellow they became of a blood-red colour—after standing for a day the precipitate was filtered out, dried in the sun, and weighed six ounces. It was then digested at a high heat, with four pounds of alcohol S.G. .837 for two hours, filtered, and again digested with two pounds more of alcohol, which, when separated by a second filtration, was added to the first. Being put into a retort, five pounds of the alcohol were distilled off, and the rest of the liquor carefully evaporated to dryness. The residuum weighed two drachms, was of a red brown colour, with

brilliant points throughout it. Its taste was sharp and styptic. It was soluble in water, and its odour like that of rhubarb. This preparation is recommended in pharmacy as being of constant quality, of convenient solubility in water, and deprived of its inert and ligneous matter—one or two grains is said to be sufficient for a dose.—*Bib. Univ.* xxii. p. 232, *and Journal of Sciences*, No. 31.

Hydrocyanate of Iron in Morbid Urine.—Dr. Julia, of Paris, has recently discovered this salt in the urine of an old man, "affected with an acute disease of the urinary passages." The urine was very viscous, frothed when shaken, had a deep blue colour, and deposited a thready sediment of the same tint. It reddened the vegetable blues, did not decompose in three days, yielded blue flakes when heated, and after evaporation and cooling formed a gelatinous mass soluble in water, and precipitating abundantly with tannin. The fluid part of this urine, therefore, contained much albumen and gelatin—but very little urea could be detected. The thready sediment weighed after desiccation about seven grains and a half. A few drops of potass, soda, or lime water, added either to this deposite or to the blue flaky matter thrown down by heat, or to the blue urine itself, speedily decolorized it—and the blue colour was at once restored on the addition of a little sulphate of iron. Dr. Julia was unable to determine what principle imparted solubility to the hydrocyanate. He observes that he has since had an opportunity of examining another sample of urine, exactly the same in nature and properties—and that a similar case occurred to a friend of his at Montpellier.—*Archives Générales de Médecine, Mai*, 1823.

MISCELLANY.

On the Preservation and Breeding of Leeches.—An interesting Memoir on this subject has been lately read by M. Noble, physician at Versailes, before the agricultural Society of the Seine and Oise—and it appears from his experiments, that these useful animals may be effectually preserved and even bred in troughs, with the aid of a little care, and a few simple contrivances. M. Noble observes, that the great mortality among them, when crowded together in small vessels, is owing to the stronger destroying the weaker for the sake of nourishment. He therefore constructed a reservoir seven feet long, three feet wide, as many deep, with sloping sides lined with clay, supplied it with a constant stream of water, exposed it to the sun, sheltered it from the north, surrounded it with a ledge of turf, and planted rushes in one of the angles. In November 1820, he placed 2000 grey and green leeches in this trough, where they

passed the winter, buried in the mud, without sustaining any loss whatever. Towards the end of spring, several young leeches were seen sticking to the old ones, and swimming round them occasionally, as if to try their strength. In August, he observed several smooth, conical holes in the mud, each of which contained a little oval cocoon, as big as that of the silk-worm, and porous outwardly like a sponge. Some of these were perforated at each end, and empty—others were filled with a transparent jelly—and the rest contained from nine to fourteen young leeches, which in a few days pierced their envelope, and swam vigorously about. M. Noble has not yet witnessed the formation of the cocoons—for as soon as the leech enters the conical holes, the water becomes muddy, and conceals their operations.

At the reading of the Memoir from which the foregoing extract is abridged, a member, M. Plancy, observed, that the formation of cocoons by the leech has been long known to the people in the department of Finistere—and that it is through means of them that the dealers who supply the capital are in the custom of stocking their ponds. The workmen dig them up from the bottom of the little muddy pools, and place them in small ponds prepared for the purpose. Six months afterwards the young are removed into larger ponds, on the edges of which cows and horses are brought to feed, for the purpose of nourishing them and hastening their growth—experience having taught the country-people, that the leech is never prolific except after having sucked the blood of animals.—*Journ. Univers. Avril,* 1823.

AMERICAN INTELLIGENCE.

The Varioloid Disease in the United States.

Concerning this disease as it has appeared in different portions of the United States, our information is not very precise. The accounts, however, furnished us by two of our correspondents at Lancaster, where it first occurred, show it under nearly the same character which it exhibits in Europe. It broke out in November 1818, and was alleged to be traced to some German emigrants, who disseminated it in passing through that city

into the interior of the country. The disease attacked indiscriminately the variolated, the vaccinated, and unprotected, though not in the same proportions. Of the first description, there were six cases, of whom none died—of the second, forty—two of whom, very young children, died in convulsions—and of the third, three hundred and fifty, among whom there were four deaths. This slender mortality, with some other facts, lead us to suspect that the disease was varicella.

From Baltimore, where it prevailed in the winter of 1822, our intelligence is still more defective. It is stated, however, to be the common impression, that the disease was imported from Liverpool, though this is doubted—and we learn, it occasionally attacked both the vaccinated and variolated, we presume in a mitigated shape, since no death occurred under such circumstances. The unprotected suffered much, many being affected, attended with a mortality of about one in six or seven cases. This is the substance of various communications which have reached us, separated from a mass of vague and contradictory statements.

As regards the disease in this city, we are not prepared at present to enter into any minute details. The facts are not sufficiently collected and ascertained, to warrant positive and satisfactory conclusions on the subject. But investigations are in progress, and when we are somewhat relieved from our oppressive duties, we shall endeavour to digest the vast materials which have accumulated in our hands, with a view to publication. To gratify, in the mean time, the curiosity of our readers, which we know to be eager and inquisitive, we now propose to lay before them some of the leading and most important facts.

As early as last June, an eruptive fever which was considered as ordinary varicella, made its appearance, and prevailed pretty extensively. During its existence, some time in July, two or three cases of a very strongly marked *variolous* character, occurred in widely separated parts of the city, the origin of which could not be traced to any source of contagion. Cases of this description rapidly multiplied, so that by the commencement of November, they had become numerous, though almost exclusively confined to Southwark, among the poorest class of our population. It was about this period, that some alarm was excited by the occasional occurrence of it, in persons who had been previously variolated, or vaccinated, and such failures daily increasing, no doubt was longer entertained, that the same epidemic which elsewhere had produced so much solicitude, had broken out among us.

The disease henceforward ran a course, and in every essential feature conformed, to what has been observed in regard

to it in other places. It attacked the variolated, the vaccinated, and the unprotected, producing those modifications under the several circumstances stated, which are described so accurately by the foreign writers, noticed in one of our preceding numbers, as to render a recapitulation here unnecessary.* But in some particulars it differed, and among which the protective power of vaccination has with us proved infinitely less. It is computed from data tolerably authenticated, that between four and five thousand failures of this process have taken place, and we have not been able to collect more than thirty or forty instances of secondary small-pox, and very few where the previous attack was in the natural way, or so violent by inoculation, as to have left any marks behind. The following table, taken from the report of Dr. Mitchell, who had charge of the Small-pox Hospital, is interesting in several views.

"*Statement of the result of one hundred and forty-eight cases of Small-pox.*

47 cases occurred in persons who had been previously affected by the vaccine disease. None of them died.

8 cases occurred in persons previously affected with small-pox—of whom four died, and four recovered.

93 cases occurred in persons who had not had either disease before—of whom 52 died, and 41 recovered.

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Of the whole number, 69 were whites, and 79 persons of colour.

Two out of the eight persons who had small-pox a second time, took it the first time naturally, or without inoculation.

Eight of those vaccinated, were vaccinated during the prevalence of the present epidemic—and some of the mildest cases were in the persons of those who had been vaccinated upwards of twenty years before."

This table includes the results only to the 14th of January. We have however learnt that the relative proportions since, in every respect, are pretty nearly the same.

The record of the Board of Health, up to the present time, shews that little more than three hundred have died of small-pox, and four only of the varioloid disease, whether the last followed variolation or vaccination does not appear. No instance has come within our knowledge of any repetition of attack in the same person, as noticed in Europe.

It must be deduced from the above facts, whatever may be our reluctance to do it, that vaccination, as a preventive of the *epidemic of this city*, has proved so inefficient as scarcely

* Vid. Review of Thompson's and Cross's work on Varioloid Disease, in our fourth volume.

to deserve to be considered at all in this light. Determining from our own observations we should say, that of any given number of individuals, however perfectly vaccinated, who might be exposed to the concentrated infection, not one-fourth would escape the disease, in some shape or degree—and in this estimate we are entirely supported by Dr. Mitchell and several other of our medical friends. Contemplated in this aspect only, variolation has, we think, a decided and indisputable superiority—in its mitigating influence an equality—though, in every other respect, its disadvantages are incomparably greater and more mischievous.

In Europe the same distrust of the security furnished by vaccination exists, as appears from many of the recent publications, and more particularly from Professor Thompson's work on Small-pox. To the paper of Dr. Gregory in our present number, we beg leave to refer for some very important and conclusive statements on this subject. It seems now, indeed, to be the creed into which the medical mind every where is fast settling down, that vaccination is chiefly valuable as preserving life, by tempering the violence of small-pox. Even thus limited, its utility is inappreciable, as will appear more conspicuously, when the fact is proclaimed, that nearly one half, according to pretty accurate reports, have died in this city of the epidemic in the unprotected system, and not more than one in the thousand, where vaccination had been received.

We cannot, at present, enter into details relative to our views of the nature of this epidemic. These will be fully developed in the work which we are very soon to give to the public. We cannot, however, forbear to state as our deliberate conviction, that the disease neither here nor elsewhere, was genuine small-pox—and to this is owing what has proved a most perplexing problem, the frequent incompetency of both variolation and vaccination to protect against it, in contradiction (supposing it to be really small-pox) to all former experience.—Editor.

Experiments, &c. with regard to the Varioloid Disease.
By W. Darrach, M. D.

Among the cases of *Small-Pox*, and those other eruptive diseases which have of late been considered modifications of it, that have fallen the last two months under my notice, while physician of the southern district of the City Dispensary, I have been able to observe the following varieties:

1. *The well known distinct and confluent Variola:* the eruption continuing in the state of pimples three and four days, be-

coming afterward large, flattened, depressed and elevated vesicles, containing lymph under the yet adhering cutis vera, then changing into spheroidal pustules, and forming ultimately circular scabs of a dark colour—which, on examination, are found to be cakes of inspissated glutenous matter, flat on its under surface, and rounded or pitted on the upper, covered with a healthy epidermis.

2. *A variety which has been fatal in every instance*—consisting of very small pustules, so contiguous that the surface of the body is made to resemble that of the prepared seal skin, very ruff, tumefied, and so painful that the patient cannot suffer the slightest touch. Death happens from excessive irritation, generally before the eruption has arrived at the suppurative stage.

3. *The Vesicular Pox.*—It has only appeared in infants, and is doubtless owing to the texture of their skin. Each pock has an irregular form, with several pits or depressions—and consists evidently of clusters of contiguous pimples, which, however, have not become confluent. The lymph, unlike the ordinary pox, is contained in distinct cells, and on being punctured exudes, as in the vaccine vesicle, in the form of dew drops.

All the cases have proved fatal before the suppurative state of the eruption.

4. *The Tuberculated Pox.*—This pox at the commencement, is a hard, distinct pimple, without much areola. For several days it preserves the tubercular character, becoming gradually harder, enlarging its base, and elevating its apex till it presents the appearance of a conical body. A vesicle of a straw colour then forms on the apex, which day after day encroaches on the side of the conical tubercle, until it has become a soft straw coloured spheroidal pustule without any areola.

These cases have all happened in coloured persons, and except in a solitary case, in a vaccinated individual. Though of longer duration than the ordinary small-pox, they have been invariably very mild cases, without fever after the eruption appeared—no anginose affection, no debility.

5. *The Dry Small-Pox.*—This variety does not differ from the ordinary variola, but in the slight areola which soon passes off, and in the paucity of the contained fluids. They are met with in old persons—and therefore all the peculiarities observable, are to be attributed to old age.

The eruptions which have lately received the name of varioloid, exhibit also several very obvious varieties.

1. That which has some resemblance to the small-pox in its form—but which differs from it in its progress, and is free from any peculiar smell and secondary fever. The eruption is elevated, flattened, and has something of a depression, but not the characteristic dark-coloured pit produced in the genuine small-

pox, as will be shown in a future paper, by an adhesion of the cutis vera to the subjacent parts. The vesicle of the varioloid is small, forms its seat sooner, and which has a form and attachment different from those of the small-pox scab.

The facts of its almost constant appearance in vaccinated individuals, and in those of them particularly who have been exposed to the small-pox, will urge us to call it modified small-pox.

2. A second variety is that which has very little resemblance, if any, to small-pox. If it be not the chicken-pox, or cow-pox, it may with great propriety be called Varicelloid. It consists at the onset of florid pimples, surrounded with an areola of an irregular oval form—the pimple is soon tipped with a transparent vesicle, which as soon becomes turbid, and of a straw colour, and before the fifth or sixth day, terminates in the formation of a strongly adhering transparent scab, of a light brown colour. This variety differs from the former in the character of the seat, being more superficial and involving the epidermis—and also in its appearing in unprotected subjects and in children, and in infants who have been recently vaccinated.

3. A third variety has appeared in a few cases, consisting of a rubeolous eruption with small distinct pimples, which, as the former eruption disappeared, advanced to become vesicular, and terminated on the sixth and seventh or eighth day, in the transparent brown scab noticed in the first variety.

The cases of this kind, which were only three, appeared in vaccinated individuals.

The structure and seat of the above variety of small-pox and varioloid diseases in the different periods of their progress, have been made the subject of anatomical investigation, and will be reserved for the report of the physician of the small-pox hospital.

It may not be uninteresting to add the results of a few experiments, which have been made with the matter from the first-mentioned variety of the varioloid.

It was inserted in the arms of two children who had not been inoculated with the cow-pox or small-pox matter. In both of them vesicles were formed which resembled those of the vaccine, until an eruption of fine pimples appeared about it. The most contiguous of these became, one after the other, confluent with the circumference of the vesicle, and gave an irregularity to it which made it resemble the vesicle produced by inoculation with the small-pox. The children were attacked with fever of two days duration, when a varioloid eruption appeared in different parts of the body.

Varioloid matter was inserted in the arms of a child who had the year past been vaccinated—and without any other effect

than the formation of a vesicle resembling that of the vaccine, which dried up before the sixth day.

The genuine small-pox matter was afterwards inserted into both arms of all the above cases inoculated with the varioloid, but without any other effect than a small pustule of a purulent character.

I now submit hastily, the results also of some late experiments on vaccination, reserving any discussion on them, or practical application of them, for a future paper.

Spurious vaccination has resulted from inoculation with the genuine vaccine, and in individuals free from any cutaneous disease. The scab formed in these cases was that common to purulent sores, irregular, crumbling, and of a yellowish colour.

In those individuals, on the contrary, who were or became afflicted with an eruption, the vaccine vesicle lost its ordinary character, and put on that of the existing disease.

This happened in the cases of two children, the one afflicted with the porrigo favora, the other with the porrigo larvalis. In both the vaccine vesicle preserved its usual appearance until the fifth and sixth day, when the edges of them became irregular, enlarged, and degenerated into the porrigenous scab, having at the centre, however, a portion which retained the character of genuine vaccine.

By future experiments we shall be able to know the effect of revaccination in these individuals—and also to determine whether the above mentioned centre portion of the scab retains the vaccine virus.

In three other cases, a spurious character was given to the vesicle by the supervention of the variolous eruption, on the fifth, sixth and seventh days after the vaccination.

In one of these cases the edges of the vesicle became confluent with the pustules, which appeared about it more thickly than elsewhere on the patient, and which soon arrested the progress of the centre portion of the vesicle.

Several unprotected individuals were inoculated with the centre and outer portions of the scab without any effect.

The second of these cases was that of an infant two weeks old, whose mother, vaccinated in infancy, was afflicted with the tuberculated pox. The infant when a week old was vaccinated—the vesicle advanced to the sixth day, when it sickened, and was soon covered with the vesicular small-pox, which immediately arrested the progress of the vaccine vesicle, and altered its appearance. The fluid which it contained three days subsequently, was inserted into the arm of an individual without effect.

The matter of the third case has not been used.

To the above cases of spurious vaccination may be added several others which appeared in individuals who had the very slight eruptive ailment.

The following are some of the results of repeated insertions of the vaccine virus in the same individuals, which will assist us in forming a more accurate opinion of the merits of vaccination.

1. In infants after a successful vaccination, the matter has been again inserted, and invariably without any local effect.

2. In others, on the contrary, the scab has been formed, which, except in its diminished size, did not differ from that produced by a primary vaccination. The size of this scab was generally inversely to the age of the individual. In adults, particularly some of them, the size was fully that of a genuine vesicle of the sixth day. In none of these cases could fever or any other constitutional affection be detected.

A third and fourth operation invariably produced the same kind of scab—but of more diminished size. No effect, as yet, has been produced by a fifth introduction of the virus.

Unprotected children were inoculated with a scab not larger than a line in diameter, which was the result of a fourth insertion of the vaccine virus in an adult. It produced genuine vaccination in them all except one. In this one, who had sores on the different parts of the body, it produced the common purulent sore.

This purulent matter which is always found in the granulated cavity on removing the scab of genuine vaccination, was inserted into the arms of two unprotected children, and produced the genuine vaccination.

The genuine vaccine virus was inserted in the udders of four cows—in two of them it produced no effect—in a third a small scab, which was found inefficient on the human subject—but in the fourth the genuine vaccine pustule, exhibiting in its stages no deviation from those in the human species.

These cows, and others not operated upon, were inoculated with the small-pox virus, but without effect.

Sulphate of Quinine.

It has within a few years been ascertained, that the bark contains a salifiable base, in which its active virtues reside. The principle is not precisely the same in the several species. The pale bark furnishes what is called *cinchonine*, and the yellow *quinine*, both of which are found in the red, and in larger quantity: hence, independently of other considerations, it must be the best.

The sulphuric acid added to this alkaline base, forms a neutral salt of great activity and usefulness. We had learnt that the sulphate of quinine was in high repute in Europe, particularly in the Parisian hospitals, in the cure of intermittents, in consequence of which, it was greatly employed in the treatment of that

disease, during its very extensive prevalence among us last autumn, and with such success, as nearly to supersede all other remedies. We have heard of no difference of opinion on the subject—every practitioner of this city, on the contrary, fully concurring in the vast superiority of its powers. It is given in pill, or solution in cinnamon water, or some other aromatic fluid, to which a few drops of sulphuric acid may be advantageously added, as rendering the solution more complete. The common dose is a grain, which is supposed to be equivalent to a drachm of crude bark, to be repeated more or less frequently, according to circumstances. We have, however, often given double this quantity at a dose, so that the aggregate amounted to a scruple a day—and on one occasion, knew eight grains to be given at once, through mistake, which effectually arrested an obstinate intermittent, without inducing any distress of stomach, or other inconvenience. Its use is restricted to the apyrexia, and in this, as well as every other respect, we conform to the rules adopted in relation to the crude medicine. Distinct from the minuteness of the dose, and facility of exhibition, this preparation is recommended by its effecting more prompt and thorough cures, so that relapses, before exceedingly frequent and troublesome, rarely happened after the introduction of it into the treatment of the disease.* It has moreover been found to display the same powers, in many of the cases to which the ordinary preparations of bark have been applied, as remittent and low continued fever, neuralgia, caphalagia, sciatica, and paroxysmal affections generally. Lately Dr. Elliotson has shown that the pure alkali, quinine, is as effectual, or nearly so, in the same dose, as the sulphate in intermittents, of which we should have doubted, on account of its insolubility, had not the fact been so well attested.†—Editor.

Professor Coxe's New Work.

Having for many years accustomed myself, whilst reading, to note down the *locality* of any thing, which at the moment particularly struck me as remarkable, or which I might wish again to recur to, by simply referring to my note book—I have gradually accumulated such a variety of references, especially in medicine, that finding them of great advantage to myself, I have imagined I might benefit the profession, and more particularly the younger members, by making a selection for publication. The utility of such a work will be apparent, when reading on any particular subject, by enabling us at once to refer to authorities on the same topic. It is still more obvious, in

* For a full account of the discovery of the Cinchonine, Quinine, &c. vid. Vol. II. of this Journal.

† Vid. Medico-Chirurg. Trans. Vol. XII. part 2.

ease of writing. The numerous isolated facts, &c. become thus placed as it were within our reach, without the trouble of seeking for them, in some measure at random, throughout the immense number of works which we possess, to the loss of much time and sometimes even unsuccessfully. We may also, by such facility of reference, be often, if disposed, enabled to seek at once, from their primary sources, the various facts, &c. which are too often palmed upon us as novelties in the present day—to the injury of science, and the due appreciation of those who have preceded us in the same pursuit.

A work like the present, it will readily be perceived, has scarcely any assignable limits. Every one may augment it, by merely interleaving it, and continuing each or any particular part from his own researches. It can at present therefore be only an approximation to what it is capable of becoming, in the hands of any one, who would exclusively devote himself to fill up the outlines thus marked out.

Without being in any manner responsible for the validity of the facts, &c. thus noted, my sole object is to save trouble to such as may desire to consult authorities on any particular topic of investigation. The above exposition will, without reference to the work itself, enable every one to judge how far it is calculated to answer the end proposed. I need only add that every edition will necessarily render it, as a book of reference, more complete by the additions it might receive—so as to render it an invaluable common-place book to every medical man. Similar works of reference in every branch of science, would unquestionably be useful, and would conjointly constitute an encyclopedia of no common interest.

I propose to designate the present work by the title of Memoria Medica, as the most appropriate that occurs to me. I have consumed but a part of my collection, leaving it for augmentation to a second edition, if such should be called for, both by what I still possess as well as from the aid I may reasonably hope to receive from such as may feel interested in it. Should such an edition be demanded, I shall endeavour to augment its utility by the addition of the etymologies and synonimes of the different articles, and shall improve it by any further suggestions which may be pointed out to me, or which may occur to myself. It is impossible, in the very nature of the work, to state its probable limits, as no standard exists by which I can form an opinion in this particular. In many instances, I may add, it will probably be found, that as the references have been noted at long and distant periods, and whilst reading different authors, the same particular is thereby pointed out; this, perhaps, is, on the whole, rather beneficial than otherwise, since all may not possess the identical works of each reference.

I. Redman Coxe, M. D.

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